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1 Introduction

The question whether economics should consider only data on choices or also non-choice data has been a hotly debated topic over the last couple of years. Critical voices have been raised, among others, by Gul and Pesendorfer (2005), Harrison (2008), and Bernheim (2009). Especially Gul and Pesendorfer (2005) declare economics to be a discipline that studies choices and not the processes which people use to arrive at these choices. This is also reflected in the title of their paper: they advocate "Mindless Economics" and claim that findings from neuroscience are worthless for economics: "[B]rain science cannot revolutionize economics because the latter[1] [*sic*] has no vehicle for addressing the concerns of economics" (p. 1–2).

This dissertation takes a stance that is in diametric opposition to the perspective of Gul and Pesendorfer (2005). Not only does it investigate people's choice processes, it also explicitly relies on two prominent types of non-choice data that Gul and Pesendorfer (2005) would deem irrelevant: response times and brain activation.

Why is it useful—and sometimes even necessary—also for economists to investigate the processes leading up to the choices that people make?

Economics usually traces human behavior back to two fundamental concepts: preferences and beliefs. It does so for several reasons, both positive and normative. The first is the desire to *explain* behavior: to find aspects that different situations have in common and/or in which they differ.

The second is that economists want to be able to *forecast* the behavior of economic agents. For instance, we want to be able to predict how changes in the economic environment—say, changes of tax rates—affect people's investment, employment, and consumption decisions (and, consequently, tax revenue).

The third reason is that many economic questions are of a normative kind: Based on an explanation of economic agents' behavior and on the ability to forecast their decisions, we want to be able to *evaluate* economic policies, and we want to be able to *make recommendations*: Should one change the tax system? Should one subsidize certain types of activities and tax others?

Especially due to the third reason, the distinction between beliefs and preferences is crucial, because only based on knowledge about people's preferences is it possible to make normative statements. However, preferences and beliefs

[1] Should say "former." Maybe, however, the statement has more truth to it in its original form.

are generally unobservable and need to be inferred from decisions that people make. This creates a situation of fundamental indeterminacy: the same behavior can be explained by different combinations of preferences and beliefs (see, e.g., Hansen, Sargent, and Tallarini, 1999).

To make these statements more tangible, consider as an example the experiment reported in Berg, Dickhaut, and McCabe (2005). Berg et al. asked the question, "Do individuals behave as if their risk preferences are stable across institutions?" They let 48 experimental subjects participate in three different types of auctions. It turns out that *all* subjects acted as if they were risk-loving in one of the auction types and risk-averse in the second. In the third type, "behavior was split between risk-loving and risk-averse bidding."

Such (seemingly) inconsistent behavior poses an intractable challenge for standard economic theory due to its exclusive reliance on preferences and beliefs. Mainstream economic theory has no means of addressing *why* preferences change in response to the environment or *how* beliefs are formed. All a standard economist can do when being confronted with the data from Berg et al. (2005) is to acknowledge *that* preferences (risk attitudes) have changed across the different auction types—or, if one assumes preferences to be fix, that subjects must have entertained "weird" beliefs that did not correspond to the actual payoffs and probabilities. There is no way of establishing a connection between the situations, unless one also considers *how* preferences *evolve* and how information (e.g., probabilities and payoffs) is *processed* by real people.

Such a situation of disconnect between economically relevant situations is not only scientifically unsatisfactory. It is also detrimental to the objective of normative economics: If we cannot be sure whether and how people's preferences change across situations or institutions, we cannot determine which policy is optimal.

It is this type of situations in which non-choice data, especially data on response times and on brain activation, can be of tremendous help: they aide us in finding out how situational influences shape human preferences and how information is processed.

The studies that form this dissertation are concerned with exactly this: by investigating the choice process, they attempt to explain behavior that is either hard to reconcile with standard approaches or for which competing explanations exist.

The first study, "Cognitive load increases risk aversion," presented in Chapter 2, is similar in spirit to the study by Berg et al. (2005) mentioned above: It establishes via a laboratory experiment that a specific change in the environment—in this case, an increase in cognitive load—had a measurable impact on subjects' risk attitudes. More important, it also relates the observed changes to existing dual-system models of decision making. The response times which were recored in addition to subjects' choices play a major role in the interpretation of the study's findings, since they support the view that decision making under risk is the product of interacting dual systems. The dual-system explanation, in turn, is grounded in evidence on the structure of the human brain that has been established previously.

The second study, "Social learning in asset markets—a peek into the herding brain," presented in Chapter 3, attempts to make the usually unobservable components preferences and beliefs observable by enlarging the analyzable dataset through the addition of measures of brain activation. In doing so, the study contributes to identifying various forces that shape how and to what extent we learn from observing the choices of other human beings.

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2 Cognitive load increases risk aversion

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Contributions: H. G., G. P. B., H. U., and H. R. H. designed research; H. G. acquired data; H. G. analyzed data; H. G. and G. P. B. wrote the text.

2.1 Introduction

Risk aversion is one of the key concepts in economic theory. According to economic theory, an agent who prefers a lottery $\boldsymbol{a}$ over all lotteries $\boldsymbol{b}$ that are mean-preserving spreads of $\boldsymbol{a}$ is risk-averse (Rothschild and Stiglitz, 1970). Without risk aversion, many economic phenomena—such as the existence of insurance, the risk premia paid on stocks vis-à-vis bonds, or the rationale of pension systems—could not be explained.

The standard way of modeling decision-making under uncertainty in economics is subjective expected-utility theory (SEUT). Within this framework, risk aversion is expressed via concave utility functions. While fruitful in many contexts and helpful as a normative model, SEUT faces limitations in explaining both field data—see, e.g., the extensive literature on the "equity premium puzzle"—and choices made by experimental subjects. From the perspective of SEUT, choices of people in experimental settings often have to be labeled as "inconsistent" or as "preference reversals." Examples include the now famous Allais paradox (Allais, 1953), the reflection effect (Kahneman and Tversky, 1979), and switches between risk aversion and risk preference (Berg, Dickhaut, and McCabe, 2005).

An approach that has been highly influential in psychology and cognitive neuroscience for years and that could help overcome the empirical shortcomings of SEUT is the "dual-system" approach. Recently, this approach has also found its way into economics in the form of "dual-self models" (e.g., Fudenberg and Levine, 2006, 2010). One particular application of the dual-system idea is the "risk as feelings" hypothesis suggested by Loewenstein, Weber, Hsee, and Welch (2001). The "risk as feelings" hypothesis postulates that both cognitive and emotional processes—effected by different systems in the human brain—

contribute to decision making under risk by evaluating the available options differentially.

Some experimental evidence supporting the idea that "cognitive" and "emotional" processes jointly shape decision making under risk has already been accumulated. The evidence indicates that the "emotional" process steers decisions in the direction of risk avoidance, while the "cognitive" process can override the emotional responses and thereby diminish risk aversion—see, e.g., Shiv, Loewenstein, Bechara, Damasio, and Damasio (2005) and Hsu, Bhatt, Adolphs, Tranel, and Camerer (2005) for brain lesion studies and Rubinstein (2007) for a response time study. This evidence will be reviewed in detail in Section 2.2.

Based on these empirical findings, an immediate implication of the dual-system hypothesis is that taxing the cognitive system by another task—so-called additional "cognitive load"—should lead to increased risk aversion, because when the cognitive system is occupied, the emotional system exerts greater influence on decision making. This prediction is also formally derived by Fudenberg and Levine (2010) within their dual-self framework.

Surprisingly, so far only one (unpublished) study has investigated this implication directly via an experiment: Benjamin, Brown, and Shapiro (2006) had Chilean high-school students choose between different lotteries, with the treatment group having to remember a 7-digit number while performing the task. The authors restricted themselves, however, to a between-subject design with a small number of subjects. In addition to that, their results are rather inconclusive: While in their sample of Chilean students they do find that an increase in cognitive load resulted in a significant increase of the number of risk-averse choices, they report having obtained a null finding in an earlier pilot study.

This shortcoming—the restriction to between-subject designs—is shared by all existing experimental studies of the dual-system approach to decision making under risk that we know of. Thus, while the results in the literature so far indicated that different people use different processes in decision making under risk, leading to different choices, one cannot yet answer confidently the question whether different processes are indeed *simultaneously* active *within* an individual. What is needed to address this question is a *within-subject* design. To provide such a design is the purpose of this study.

We tested 41 subjects in a laboratory experiment. Each subject completed 120 trials. In each of the trials, subjects were asked to choose one out of two lotteries offered to them in the respective trial (Random Lottery Pairs procedure). During half of the trials, subjects engaged in a cognitively demanding distractor task on top of the lottery choice.

We are the first to show *within-subject* that cognitive load increases risk aversion. We do so by observing that subjects switched significantly more often from the riskier to the less risky lottery when cognitive load was increased than in the opposite direction. We confirm this finding by computing several structural regressions to assess the quantitative change in the degree of relative risk aversion induced by the cognitive-load manipulation. All estimates of this parameter turned out to be significantly positive. Importantly, we also observe within-subject that across load levels, response times increased when subjects chose the riskier of the two lotteries offered to them in a given trial. Only now is Rubinstein's (2007) claim that risk aversion partially results from "instinctive reasoning," which he made based on between-subject findings, warranted.

Both findings provide evidence that the dual-system approach to decision making under risk can explain not only individual differences in risk attitudes but also within-subject variation in response to certain situational influences. Especially the observed response time pattern is incompatible with a "unitary process" approach.

In the remainder of this paper, we first clarify terms ("multiple-process," "multiple-system," "dual-process," "dual-system," "emotions") and very briefly review the dual-system approach in general. We then present in detail the existing empirical evidence that forms the background of this study by relating decision making under risk to the dual-system hypothesis. After that, the experimental design of our study is described in detail, followed by the estimation procedures and the results.

2.2 Related literature

2.2.1 Introductory remarks

Our aim in this section is to provide readers from different disciplines with a sketch of the background of this study. This section, therefore, both clarifies terms like "multiple-process," "multiple-system," "dual-process," "dual-system," and "emotions" (Section 2.2.2)—which are uncommon in economics—and touches upon subjective expected-utility theory (Section 2.2.3)—the standard way of modeling decision making under uncertainty in economics. We, thereby, briefly review the dual-system approach in general (Section 2.2.2), its relation to decision making under risk (Section 2.2.4), and then summarize in detail the existing empirical evidence on dual systems being involved in decision making under risk (Section 2.2.5).

2.2.2 Overview of dual-system and "dual-self" approaches

The theory that is by far most commonly used in economics for modeling decision-making under risk is subjective expected-utility theory (SEUT). The virtues of SEUT are hardly debatable: It provides a unifying framework for the analysis of a wide array of situations; it permits developing—relatively—

simple models, compared to alternative approaches like ambiguity aversion or prospect theory; and since it is based on intuitively appealing axioms of what constitutes rational behavior, it serves as a widely accepted normative model.

However, SEUT faces limitations in explaining field data as well as choices made by experimental subjects. According to SEUT, choices that people make in experimental settings often have to be classified as "inconsistent." Due to its reliance on axioms of rational behavior and its assumption that decision makers obey the laws of logic and probability, SEUT has no means of addressing such "inconsistent" choices.

To explain observed behavior that SEUT cannot explain, but which may nevertheless be systematic, several alternative theories of decision making under uncertainty have been developed — among them ambiguity aversion (in different flavors), prospect theory, and heuristics-based models of the evaluation of uncertain payoffs. One particular alternative theory is the so-called "multiple-process approach;" it postulates that choices between different uncertain payoffs (actually, decision making in general) are the outcome of — at least two — separate, but possibly interacting, processes which evaluate the available options.

Multiple-process approaches have a long history in psychology:

> There is a long legacy of research within psychology, strongly supported by findings from neuroscience, to suggest that human behavior is not the product of a single process, but rather reflects the interaction of different specialized subsystems. (Sanfey, Loewenstein, McClure, and Cohen, 2006, p. 111.)

Evans (2008) reviews theoretical developments, empirical evidence, and limitations concerning multiple-process approaches in various domains of decision making. One of the themes that emerge — and that is also expressed in the above quote of Sanfey et al. (2006) — is that the different processes are postulated to be run by different systems in the brain. These systems, in turn, are then often postulated to be associated with different brain areas (i.e., a so-called structure–function mapping is proposed). For this reason, the terms "multiple-process" and "multiple-system" are often used interchangeably.

Many contributions to the literature take a simplifying stance and reduce the assumed number of involved processes/systems to two, so that the "multiple-system approach" in most cases boils down to a "dual-system approach." The most neutral terms found in the literature to label these systems are simply "System 1" and "System 2." Evans (2008) finds the characteristics attributed to the two systems to be very similar across the vast majority of publications using dual-system approaches:

System 1	System 2
Unconscious (preconscious)	Conscious
Automatic	Controlled
Low effort	High effort
Rapid	Slow
High capacity	Low capacity
Default process	Inhibitory
Evolutionarily old	Evolutionarily recent
Domain specific	Domain general
Pragmatic	Logical
Parallel	Sequential
Stereotypical	Egalitarian
Universal	Heritable
Independent of general intelligence	Linked to general intelligence
Independent of working memory	Limited by working memory capacity

Table 2.1: Some of the labels attached to dual systems of decision making in the literature reviewed by Evans (2008).

> Almost all authors agree on a distinction between processes that are unconscious, rapid, automatic, and high capacity [System 1], and those that are conscious, slow, and deliberative [System 2].

Specific features attributed to the two systems in the studies reviewed by Evans are listed in Table 2.1.

One distinction that is crucial for our study is that, in contrast to the "high capacity" nature of System-1 processing, System-2 processing seems to be limited by access to working memory (Evans, 2008, p. 261): "It appears that conscious thought is inherently sequential, whereas many theorists suppose the rapid processing and high capacity of System 1 reflects use of parallel processes." In line with this is the finding that "[w]orking memory capacity is known to predict [between-subject] performance levels in a very wide range of cognitive tasks and has been directly linked with dual-process accounts of cognitive functions" (Evans, 2008, p. 262). The importance of these observations for our study stems from their implication that if decisions under risk are generated by an interaction of System 1 and System 2, we should be able to manipulate risk attitudes via a task that taxes working memory.

Models related to the dual-system approach, dubbed "dual-self models," have recently been introduced to the economic literature—mostly to explain phenomena related to intertemporal choice (temporal discounting), e.g., Bernheim and Rangel (2004), Loewenstein and O'Donoghue (2005), and Fudenberg and Levine (2006). In the meantime, dual-self models have also been applied to phenomena related to risk aversion (Fudenberg and Levine, 2006, 2010). Fudenberg and Levine (2006) consider it a virtue of their theoretical model that it bridges the gap between thus far disparate phenomena: They argue that their "simple 'dual-self' model gives a unified explanation for several empirical regularities" from both the domain of intertemporal choice and decision making under risk (p. 1449). Notably, their model predicts that risk attitudes change when agents carry higher cognitive load.

2.2.3 Subjective expected-utility theory as a unitary-process model of decision making under risk

Loewenstein, Rick, and Cohen (2008, p. 647) make the bold claim that it is "the bedrock assumption within economics that decision making is a unitary process—a simple matter of integrated and coherent utility maximization." But is it really obvious that subjective expected-utility theory (SEUT) is to be categorized as a unitary-process model?

This question arises because SEUT is, strictly speaking, a theory of choice *outcomes*—and not of *how* agents choose. That is, it does by itself not make any statement on the decision-making *process(es)* involved. One could, thus, argue that classifying SEUT as a "unitary-process" model makes little sense because the classification is based on concepts that SEUT says nothing about.

SEUT does predict, however, regularities that agents' *choices* should obey. These predictions are derived from the premise that choices are influenced by the criteria which SEUT assumes to be relevant, specifically the probability and the magnitude of potential payoffs, in a coherent way. For this reason SEUT—in its basic form—rules out situational characteristics to have an influence on choices. More specifically, even though SEUT in general permits several evaluation processes to contribute to decision making, it does entail the assumption that the interaction of the involved processes is situation-invariant. Thus, one may view the potential conglomerate of processes as a single process, because its characteristics do not change in response to situational changes. Consequently, Loewenstein et al.'s (2008) categorization of SEUT as a "unitary" approach is justified and sensible.

As already mentioned, the predictions of SEUT have been frequently shown to be violated. Among others, situational characteristics—e.g., the incidental mood (affect) of subjects (Isen and Patrick, 1983; Isen, Nygren, and Ashby, 1988; Nygren, Isen, Taylor, and Dulin, 1996), prior gains and losses (Thaler and Johnson, 1990; Shiv et al., 2005; Weber and Zuchel, 2005), as well as cognitive load (Benjamin et al., 2006)—have been demonstrated to lead to changes in experimental subjects' evaluation of stochastic payoffs.

It is, of course, possible to extend SEUT to capture situational influences. This could simply be achieved by introducing state-dependence of the utility function. Without any further foundation, introducing state-dependent utility would, however, be highly unsatisfactory: It would merely add a degree of freedom to the model, such that its ability to fit observed behavior is enhanced. What it would fail to do is to *explain* behavior, because no *reason* would be given as to why the situational characteristics influence subjects' choices in the way they do. The different situations would stand side by side unconnectedly. Such a situation of disconnect is, however, not desirable scientifically.

Exactly such a situation is what our present study attempts to resolve: By showing that risk attitudes are state-dependent *and* by showing that cognitive load is a dimension in which risk attitudes vary significantly, for which the dual-system approach offers a theoretical explanation, we provide a "micro-foundation" for this very state dependence.

2.2.4 Dual-process approaches to decision making under risk

As explained in the previous section, it makes sense to see a fundamental difference between SEUT—unless augmented by a state-dependent utility function—and models that suppose an interaction between "System 1" and "System 2." Let us now turn to a type of dual-system approaches that is especially relevant for decision making under risk: the interaction of emotions and deliberation.

Emotions are classified as a special type of System-1 processes (see Evans, 2008, p. 256/258). When we speak of emotions in this paper, we use the term as defined by Sanfey et al. (2006, p. 111):

> For present purposes, we will use 'emotion' to refer to low-level psychological processes engaged by events that elicit strong valenced and stereotyped behavioral responses (...). Accordingly, emotions are rapid, highly automatic responses to specific stimuli or events, well adapted to some circumstances but not to others.

Emotions explicitly play a role in a prominent multiple-process account of decision making under risk: the "risk-as-feelings" hypothesis developed by Loewenstein et al. (2001) (in contrast to "risk as analysis," Slovic, Finucane, Peters, and MacGregor, 2004). The main tenet of the "risk-as-feelings" hypothesis is that besides a "cognitive evaluation" of a risky situation in line with or at least similar to SEUT, "responses to risky situations (...) result in part from direct (i.e., not cortically mediated) emotional influences, including feelings such as worry, fear, dread, or anxiety" (Loewenstein et al., 2001, p. 270).

Loewenstein et al. (2001) suggest that emotions in response to a risky situation may arise irrespective of the cognitive evaluation (p. 270):

> [F]eeling states are postulated to respond to factors ... that do not enter into cognitive evaluations of the risk and also respond to probabilities and outcome values in a fashion that is different from the way in which these variables enter into cognitive evaluations.

Thus, in some situations the two modes of evaluation of risky options may diverge, with behavior being "then determined by the interplay between these two, often conflicting, responses" (p. 270).

This might make a person's risk attitudes vary considerably across situations, a point that Cohen (2005) and Sanfey et al. (2006, p. 111) also subscribe to:

> Although most of the time these systems interact synergistically to determine behavior, at times they compete, producing different dispositions towards the same information.

2.2.5 Empirical evidence on dual processes in decision making under risk

Introductory remarks

The empirical evidence that points to decision making under risk being the outcome of at least two interacting processes can be categorized as follows:

1. direct evidence from brain lesion studies and neuroimaging studies;
2. direct evidence from studies on response times;
3. indirect evidence from related research areas;
4. influence of cognitive load on decision making under risk.

In the following, we review studies from all four categories.

Direct evidence from brain lesion studies and neuroimaging studies

The probably most convincing piece of evidence is provided by Shiv et al. (2005). They find that in their experiment, subjects with lesions "in specific components of a neural circuitry that has been shown to be critical for the processing of emotions"—the amygdala, anterior insula, or orbitofrontal cortex (OFC)—made significantly *less* risk-averse choices than control subjects (with lesions in the dorsolateral prefrontal cortex) and than normal participants. Consequently, lesion patients earned a significantly larger amount of money on average compared to healthy subjects and control patients.[1]

Crucially, from a theoretical perspective, the lesion patients' choices could even be labeled as being more consistent between-trials than control subjects' choices. This is important because it indicates that the loss of the function of the lesioned brain regions does not lead to a general impairment of the patients' decision making, i.e., the lesions do not result in random choice.

At the same time these patients do not seem to have just mechanically picked the option with the higher expected value, but some sense of aversion

[1] A weakness of the study by Shiv et al. (2005) is that from their experiment one cannot conclude whether lesion patients were actually risk-neutral or risk-seeking.

to risk seems to have been maintained. This is important because it allows us to still legitimately speak of a "choice," in which pros and cons were weighed against each other.[2] Taken together, this indicates that indeed at least two decision-making processes are at play: an "emotional" one that was disrupted by the lesions and that normally leads to caution in the face of risk, and another, "cognitive," one that rather adheres to risk neutrality.

Similiar evidence is reported by Hsu et al. (2005): They compare choices of patients with OFC lesions in ambiguous and risky situations to those of control subjects who had lesions in other brain areas. Consistent with Shiv et al.'s 2005 findings, Hsu et al. (2005) observe patients with OFC lesions to be significantly less risk-averse (and less ambiguity-averse) than control subjects.

The observations by Shiv et al. (2005) and Hsu et al. (2005) suggest that the brain areas amygdala, orbitofrontal cortex, and right insular cortex contribute to decision making in a way that the decision maker exhibits risk aversion. Based on a meta-analysis of studies that used a different methodology—functional magnetic resonance imaging (fMRI), a neuroimaging technique to measure and locate activation in the entire brain—Mohr, Biele, and Heekeren (2010) arrive at similar conclusions. They suggest the following mechanism (p. 6618):

> [W]hen individuals observe a risky stimulus such as a gamble with uncertain outcomes or an investment option, two parallel and reciprocal risk processes are induced, an emotional and a cognitive risk process. On the emotional level, activity in the aINS [anterior insula] initially serves as a fast and rough estimate for the potential of the stimulus to result in an unwanted outcome (e.g., a loss).

This, of course, raises the question, how the outcomes of the two processes are combined. Mohr et al. (2010) suggest that the emotional and the cognitive evaluation get integrated as follows:

> The DMPFC [dorsomedial prefrontal cortex] evaluates the risk of the stimulus on a cognitive level, for instance, computing the variance of outcomes or the probability of a loss, thereby using the information from the aINS and the thalamus as a first estimate for the riskiness of the stimulus. During this process, information is repeatedly exchanged between DMPFC on the one hand and aINS and thalamus on the other hand, updating the emotional response to the stimulus, which in turn informs the cognitive processing of risk.

[2] Admittedly, the basis for this claim would be stronger if we knew that subjects behaved as reported by Shiv et al. (2005) also individually.

According to Mohr et al. (2010, p. 6618), "[t]he mechanism proposed here is compatible with the general approach of the risk-as-feelings hypothesis" (Loewenstein et al., 2001).

Direct evidence from studies on response times

In a between-subject analysis of an Internet-based experiment in which subjects (n = 2,426) were asked to choose between two (hypothetical) gambles, Rubinstein (2007) finds that decisions to choose the less risky of the two gambles were made substantially faster than decisions to choose the riskier of the two gambles (see his Table 8 on p. 1256).

There are two possible explanations for this observation: It could be that subjects with higher cognitive ability solved the lottery choice task faster, and that cognitive ability covaries with risk attitudes (see the evidence presented below in Section 2.2.5). It could also be—and this is Rubinstein (2007)'s interpretation—that this between-subject finding reveals different modes of reasoning that are also present within-subject: Rubinstein (2007) calls them "cognitive" and "instinctive."

If also a *within-subject* study revealed that risk-avoiding choices were made faster than risk-accepting choices, this would clearly speak in favor of multiple evaluation processes[3]: It would support the idea that the "emotional"/"instinctive" process quickly guides the decision in the risk-averse direction and that the slower "cognitive" process can overrule this tendency.

Within-subject differences in response times that depend on the choice made would be strong evidence against a unitary evaluation process: Under a unitary process, no difference in response times, depending on the choice taken, should be observable. Of course, it is true also under a unitary process that more complex lotteries, being harder to evaluate, should prolong the time until a choice is made—but for a given degree of task difficulty, the computations needed until an informed choice can be made are identical, no matter what the result of those computations; hence, response times should *not* depend on the decision made if a unitary process is at work.

Indirect evidence from related research areas

Cognitive ability and preferences. Dohmen, Falk, Huffman, and Sunde (2010) analyze whether people's degrees of risk aversion and their degrees of patience

[3] An exception are heuristics-based descriptions of lottery choice, such as the priority heuristics proposed by Brandstätter, Gigerenzer, and Hertwig (2006). Even though one might classify the priority heuristic as a unitary-process model, it is capable of predicting shorter response times for risk-avoiding choices. However, apart from being descriptively lacking in some respects, the priority heuristic needs to be augmented in a dual-system fashion to account for the alteration of behavior that the lesions described in Section 2.2.5 cause. A more detailed discussion follows in Section 2.5.

covary with their cognitive abilities. Dohmen et al. find that people with higher cognitive abilities are on average less risk-averse. These results are confirmed for different subject pools by Benjamin et al. (2006), Burks, Carpenter, Goette, and Rustichini (2009), and Oechssler, Roider, and Schmitz (2009).

Given that "[i]t is now well established that individual differences in working memory capacity and general intelligence measures are very highly correlated" (Evans, 2008, p. 262), these results are in line with the dual-system hypothesis that ascribes the role of steering choices in the direction of risk neutrality to the working-memory-dependent System 2. Since other forms of processing (System 1) seem to be dissociated from general intelligence, Evans (2008, p. 262) even claims that "one of the stronger bases for dual-systems theory is the evidence that 'controlled' cognitive processing correlates with individual differences in general intelligence and working memory capacity, whereas 'automatic' processing does not."

The piece of evidence that completes the picture is the finding that risk attitudes are heritable (Dohmen, Falk, Huffman, and Sunde, 2011a). It fits the picture because risk attitudes were found to covary with cognitive ability (Dohmen et al., 2010), which, in turn, is heritable (compare Table 2.1).

Intertemporal choice. Dual-system approaches have also been recruited to explain phenomena in the economically relevant field of intertemporal choice. They have mainly been employed to provide a foundation of hyperbolic discounting. Notably, however, both on the behavioral level (does cognitive load lead to stronger temporal discounting?) and concerning the neural basis of temporal discounting, the experimental results are thus far inconclusive (see Hinson, Jameson, and Whitney, 2003; Franco-Watkins, Pashler, and Rickard, 2006; Hinson and Whitney, 2006; Franco-Watkins, Rickard, and Pashler, 2010; Getz, Tomlin, Nystrom, Cohen, and Conway, 2010, for the influence of cognitive load; see McClure, Laibson, Loewenstein, and Cohen, 2004, and McClure, Ericson, Laibson, Loewenstein, and Cohen, 2004, vs. Kable and Glimcher, 2007, and Peters, 2011, for the neural level).

Development psychology. Steinberg (2008) cites evidence that the willingness of adolescents to take (potentially very harmful) risks is elated, compared to children and adults. He explains the increased risk-taking by adolescents through the interaction of a "cognitive control system" and a "socio-emotional system." He provides evidence that during the development from childhood to adulthood, areas implicated in cognitive control do not develop in lockstep with areas implicated in emotional processing.[4]

[4] However, contrary to our hypotheses, in Steinberg's (2008) view it is the engagement of the "cognitive control system" that leads to *less* risk-taking, while the "socio-emotional system" favors risk-taking, because it is susceptible to the attraction exerted by the potentially high rewards of a risky situation. This leads to an interesting possibility: It might be the case that the "emotional system" does not produce risk-taking or risk-averse responses across the board,

Influence of cognitive load on decision making under risk

Thus far, there is a single experimental study that has attempted to establish a direct link between cognitive load and risk attitudes: Benjamin et al. (2006). While the focus of that paper is on linking individuals' degrees of small-stakes risk aversion to their general cognitive abilities, the authors also provide some evidence that risk attitudes can be influenced through the use of "higher-order cognitive processes" and by increased cognitive load: In one of Benjamin et al.'s (2006) experimental conditions, subjects had to make the reasons for their choices explicit (i.e., verbalize them), resulting in *less* risk aversion. In a different experimental condition, participants were subjected "to a cognitive load manipulation designed to decrease working memory"; this *increased* participants' small-stake risk aversion.

The study by Benjamin et al. (2006) is very similar to our study in spirit. Our study, however, offers several improvements over their design.

1. First of all, their results have not been replicated yet. To establish robustness of findings, replication is essential in experimental economics anyway, but it is especially warranted in this case, because Benjamin et al. (2006) "note that a pilot study using Harvard undergraduates as participants failed to find any significant effect of cognitive load on expressed preferences" (p. 27).
2. Their findings are based on a between-subject design with a single decision made by each subject. Our study features a within-subject design, with multiple choices made by each subject, i.e., a much larger number of observations.
3. The low number of observations in Benjamin et al. (2006) precludes quantitative analysis of the influence of the cognitive-load manipulation on preference parameters. While they do test whether the choice probabilities differ significantly between conditions, their design does not allow for relating these choice probabilities to preference parameters. In contrast, the multiplicity of choices per subject in our study enables us to estimate the change in the degree of relative risk aversion induced by the cognitive-load manipulation.
4. Their reliance on a single choice per subject also precludes estimating whether subjects' choices became less consistent with increased cognitive load.

but that it simply produces quick, impulsive responses. For some people (or many people at a certain age) the impulsive response might be risk-seeking, while for others it is risk-averse. The role of the "cognitive control system" might then be to inhibit and deliberate on the impulsive response, with the effect that the impulsive response is attenuated: Risk-averse people get less risk-averse, risk-seeking people get less risk-seeking.

5. We record not only choices but also response times, since these provide information that is crucial for drawing conclusions as to whether multiple processes are involved in subjects' decision making under risk—see Section 2.2.5.

2.3 Experimental design

2.3.1 Introduction: Advantages of our design over alternative designs

The general idea is to present subjects repeatedly with choices between different lotteries, so that one can make inferences from their observed choices on their risk attitudes. During one condition in the experiment, subjects performed an additional task that taxed working memory. The intention was to find out whether the reduction in working memory available for carrying out the lottery choice that was induced by the additional, simultaneous task resulted in a significant change (increase) of subjects' degree of risk aversion. Let us elaborate on some of the specifics of the experiment:

1. For the lottery choice task, we used a variant of the Random Lottery Pairs procedure (explained in Section 2.3.2) popularized by Hey and Orme (1994). The advantage of this procedure over other procedures, such as the "Multiple Price List" oder the "Iterated Multiple Price List" design, is that the latter would have facilitated remembering previous choices, thus making choices across the two conditions non-independent.
2. Subjects received remuneration for the lottery choice based on exactly one randomly selected trial. This is necessary for measuring the degree of risk aversion correctly, because the estimation procedure relies on the observed choices to be independent across trials. Thus, subjects must not be given the opportunity to hedge their decisions across trials. Exactly such hedging is possible if multiple trials are payoff-relevant, because subjects then in fact face a compound lottery.

   This is exactly what a design like the one by Shiv et al. (2005) does, in which subjects were paid their accumulated payoffs. As already stated, in such a design estimating the degree of relative risk aversion becomes impossible. The same is true about the "Balloon Inflation Task" used by Rao, Korczykowski, Pluta, and Detre (2007): The step-by-step inflation of the balloon also amounts to confronting subjects with a complicated multi-stage lottery in each trial.
3. In addition to the lottery payoff, subjects received a reward of €5 upon answering correctly in the working-memory task. Here, too, it is crucial

for correct measurement of the risk attitude, that the reward for the working-memory task and the payoff stemming from the lottery choice are *independent of each other*.[5]

4. The distractor task used by Benjamin et al. (2006) required subjects to remember a 7-digit number while making the lottery choice. We decided to use a different cognitive-load manipulation: a spatial working-memory delayed-matching task (a detailed description of the task follows below, in Section 2.3.2). Given that we wanted to generate a large number of observations per subject, the spatial working-memory delayed-matching task is preferable to the number task, because the memorizing phase is much shorter, so that trial duration remains short. In addition, also the answering phase is short and requires only a single button press (for "yes"/"no".)

2.3.2 Trial setup

Lottery choice task. We used a variant of the Random Lottery Pairs procedure popularized by Hey and Orme (1994): In each trial, subjects were shown a lottery pair $(\boldsymbol{a}_t, \boldsymbol{b}_t)$ out of a set of 60 lottery pairs. The lottery pairs were presented in pseudo-random order. Each lottery $\boldsymbol{l}$ consisted of two possible payoffs $(x_{\boldsymbol{l},1}, x_{\boldsymbol{l},2})$ and was illustrated by a pie chart visualizing the probabilities $(p_{\boldsymbol{l}1}, p_{\boldsymbol{l}2}) = (p_{\boldsymbol{l}1}, 1 - p_{\boldsymbol{l}1})$ associated with the possible payoffs (see Figure 2.1). This graphical representation is common in this type of experiments (see Harrison and Rutström, 2008).

Subjects were asked to choose one of the two offered lotteries within a time frame of 6.5 sec. The lotteries included in a pair differed from each other in the expected value and in the variance: In most cases, the lottery with the

[5] A possible alternative payoff structure would have been the one used by Benjamin et al. (2006), i.e., the winnings from the lottery are only realized in case the subject answers correctly in the working-memory task. *For a rational subject*, this should have no influence on the lottery choice, since all lotteries' probabilities are simply scaled down by a common factor: the probability of giving the right answer in the working-memory task. A benefit of this remuneration scheme would be that it prevents subjects from playing an "either–or" strategy (i.e., focussing solely on the lottery choice or on the working-memory task). However, it would introduce additional and unobservable variation across trials: For instance, subjects might reduce their attention in trials in which they do not expect to answer correctly and, thus, make less well-considered choices in those trials. This would introduce a confound, since less well-considered—and, thus, potentially less consistent—choices would primarily occur in trials with high working-memory load. Thus, we could no longer tell apart whether less consistency in choices resulted from increased working-memory load *per se* or from reduced attention due to a reduced likelihood of the current lottery choice being relevant. This is why we want the reward for the working-memory task and the payoff stemming from the lottery choice to be independent from each other. The potential problem of subjects applying an "either–or" strategy can probably be overcome by making the payoff for answering correctly in the working-memory task proportional to the expected value of the lottery pair (or to the larger EV of the two lotteries).

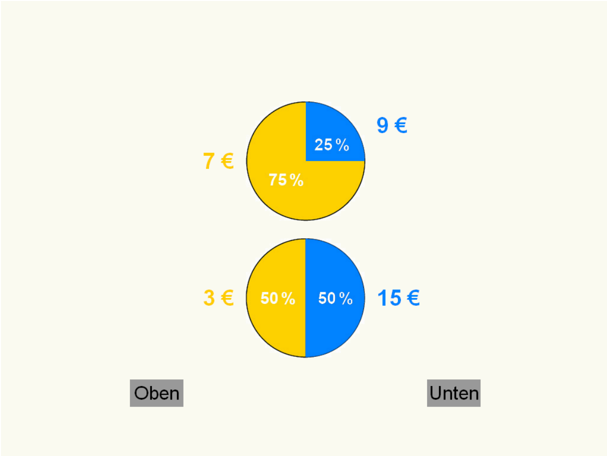

Figure 2.1: Display of an example lottery pair in the lottery choice task.

higher expected value (EV), let us call it ***a***, also featured a higher variance. This ensured that subjects had to weigh the larger average payoff of ***a*** against the larger downside risk of ***a*** compared to ***b***. (Apart from the "catch trials," we ensured that the lottery with the higher EV did not first-order stochastically dominate the other lottery, see below.) For simplicity, we will refer to the lottery with the higher variance as the "riskier" lottery, even though this is not exactly correct, according to Rothschild and Stiglitz (1970).

Positioning of the lotteries on-screen was counterbalanced within-subject: In half of the trials, the riskier lottery was presented in the upper half of the screen, and in half of the trials in the lower one. Moreover, we counterbalanced the larger payoff's position on the screen between-subject: For half of the subjects, the larger payoff was always illustrated by the left side of the pie chart; for the other half, it was always illustrated by the right side of the pie chart. Hence, if any laterality or color preference of subjects existed and had an influence on their choices, we would be able to pick up the effect by including a side dummy in our regressions.

Cognitive-load manipulation. As the cognitive-load manipulation we chose a spatial working-memory delayed-matching task: In half of the trials, subjects were briefly (1 sec) shown an arrangement of points (as in Nagel, Preuschhof, Li, Nyberg, Bäckman, Lindenberger, and Heekeren, 2009), before being presented the lottery pair (see Figure 2.2). The arrangement of points presented ("sample points") varied across trials. The locations of the different points were determined by placing them on virtual radii around the fixation cross shown at the center of the screen. Subjects' task was to remember the sample points' arrangement while choosing between the two lotteries.

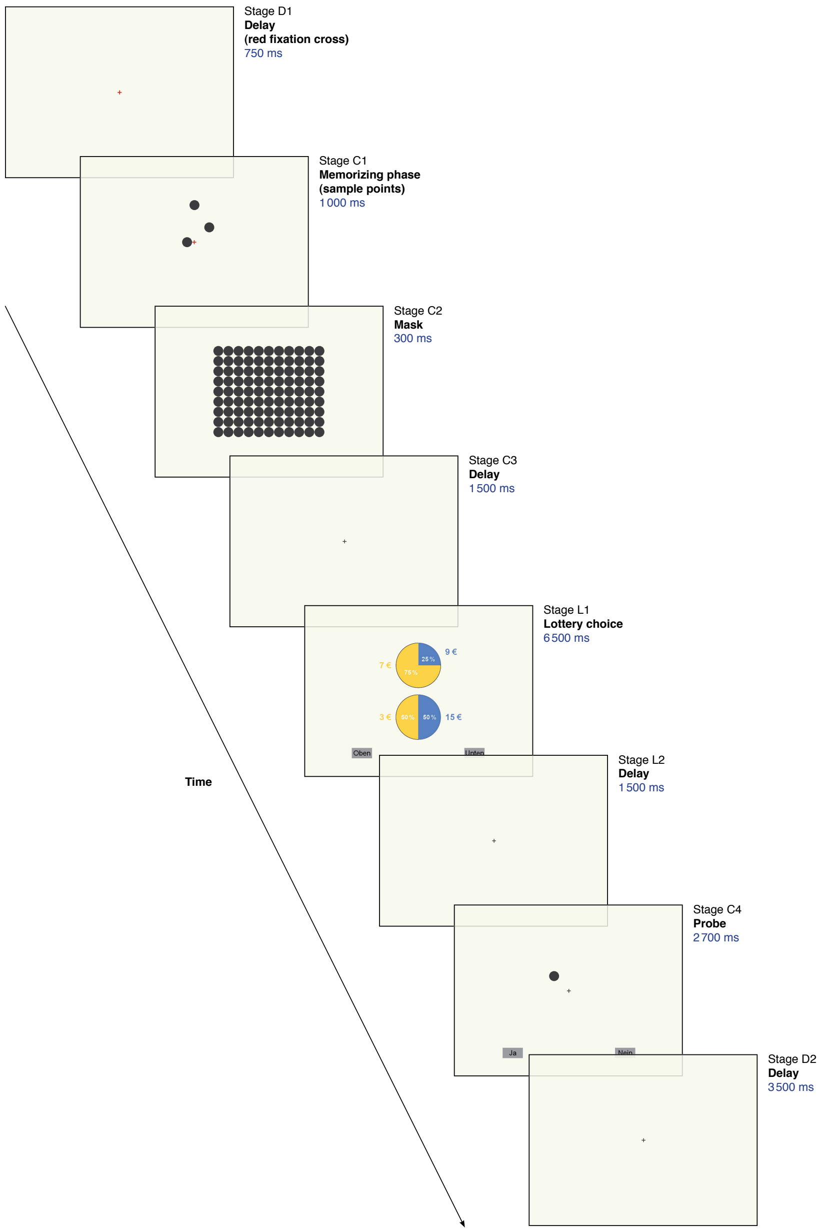

Figure 2.2: Trial setup in the condition with both the lottery choice and the working-memory task.

After the subject had chosen a lottery, one single point ("probe point") was displayed. Subjects had to indicate via button press whether or not the location of the probe point corresponded to the location of one of the sample points. To avoid ambiguity in the correct categorization, the probe was placed such that it occupied either the exact same spot as one of the sample points or a non-overlapping position, like in Nagel et al. (2009).

Catch trials. We included trials in which one lottery first-order stochastically dominated the other one as "catch trials." These enable us to assess subjects' rationality and alertness.

Preference reversals. The lottery pairs presented to subjects were chosen as follows: We assumed that in each condition i, subjects behaved in line with expected-utility maximization under constant relative risk aversion (CRRA) over the experiments' payoffs. That is, we effectively assumed subjects to exhibit state-dependent coefficients of relative risk aversion, ρ_i, with the state being the amount of working memory available in condition i. Hence, their behavior should be well described by a state-dependent utility function that assigns utility

$$u(x;\rho_i) \equiv \begin{cases} \dfrac{x^{1-\rho_i}-1}{1-\rho_i} & \text{if } \rho_i \neq 1 \\ \ln x & \text{if } \rho_i = 1 \end{cases}$$

to a payoff of €x. That is, $\rho_i = 0$ corresponds to risk neutrality, $\rho_i < 0$ to risk seeking and $\rho_i > 0$ to risk aversion.

We generated 48 lottery pairs $(\boldsymbol{a}_t, \boldsymbol{b}_t)$. Let $\boldsymbol{a}$ be the riskier lottery in an arbitrary trial. The set of lottery pairs was assembled such that for each $\rho \in \{0.1, 0.2, \ldots, 0.9\}$ there would be at least one lottery pair $(\boldsymbol{a}, \boldsymbol{b})$ for which Lottery $\boldsymbol{a} \succ$ Lottery $\boldsymbol{b}$, while Lottery $\boldsymbol{a} \prec$ Lottery $\boldsymbol{b}$ for $\rho + 0.1$. In addition to that, 8 lottery pairs were generated such that one lottery first-order stochastically dominated the other one, and 4 lottery pairs were designed such that one lottery would be preferred for any degree of risk aversion (any ρ) under CRRA preferences. The complete set of 60 lottery pairs is listed in Table 2.2.

The aim of selecting the lottery pairs in this way was to ensure the following:

1. Estimation of the coefficient of relative risk aversion should be relatively exact in the range $\rho \in (0.1, 0.9)$.
2. The lotteries included in the selected pairs would likely be similar to each other in terms of participants' subjective valuation. We presented the same 60 lottery pairs in both conditions, i.e., under both load levels (see below). Let $(\boldsymbol{a}, \boldsymbol{b})$ be one of these 60 lottery pairs. Assuming $\boldsymbol{a}$ to be the riskier lottery, what we expected to observe were preference reversals for several lottery pairs $(\boldsymbol{a}, \boldsymbol{b})$ of the kind that under the degree of risk aversion $\rho_{\mathrm{CL_{no}}}$, Lottery $\boldsymbol{a} \succ$ Lottery $\boldsymbol{b}$, whereas under $\rho_{\mathrm{CL_{yes}}}$, Lottery $\boldsymbol{a} \prec$ Lottery $\boldsymbol{b}$.

No.	Lottery $\boldsymbol{a}$ $x_{\boldsymbol{a},1}$	$x_{\boldsymbol{a},2}$	$p_{\boldsymbol{a},1}$	Lottery $\boldsymbol{b}$ $x_{\boldsymbol{b},1}$	$x_{\boldsymbol{b},2}$	$p_{\boldsymbol{b},1}$	$\boldsymbol{a} \succ \boldsymbol{b}$ for $\rho \leq$	$\boldsymbol{a} \prec \boldsymbol{b}$ for $\rho \geq$
1	5	10	0.25	6	9	0.10	0.1	0.2
2	3	15	0.50	6	9	0.10	0.1	0.2
3	3	15	0.50	5	10	0.25	0.1	0.2
4	2	20	0.50	4	12	0.25	0.2	0.3
5	3	15	0.50	7	9	0.25	0.2	0.3
6	2	20	0.75	5	10	0.90	0.2	0.3
7	2	20	0.75	3	15	0.75	0.2	0.3
8	7	12	0.75	8	10	0.90	0.2	0.3
9	3	15	0.50	7	12	0.75	0.3	0.4
10	6	15	0.75	7	9	0.50	0.3	0.4
11	6	15	0.75	8	8	1.00	0.3	0.4
12	3	15	0.50	8	10	0.90	0.3	0.4
13	3	15	0.50	6	9	0.25	0.3	0.4
14	3	15	0.75	5	10	0.90	0.3	0.4
15	2	20	0.50	5	10	0.10	0.3	0.4
16	6	15	0.75	6	10	0.50	0.4	0.5
17	3	15	0.50	8	8	1.00	0.4	0.5
18	3	15	0.50	6	15	0.75	0.4	0.5
19	6	12	0.50	7	9	0.10	0.4	0.5
20	8	15	0.75	6	10	0.10	0.4	0.5
21	3	15	0.50	7	9	0.50	0.4	0.5
22	4	12	0.50	7	9	0.75	0.4	0.5
23	3	15	0.25	11	11	1.00	0.5	0.6
24	6	9	0.25	8	10	0.90	0.5	0.6
25	3	15	0.25	4	12	0.10	0.5	0.6
26	4	12	0.50	6	9	0.50	0.5	0.6
27	4	12	0.10	11	11	1.00	0.5	0.6
28	4	12	0.50	7	12	0.90	0.5	0.6
29	4	12	0.25	6	10	0.10	0.5	0.6
30	3	15	0.50	6	9	0.50	0.6	0.7
31	3	15	0.50	7	9	0.75	0.6	0.7
32	6	12	0.50	6	9	0.10	0.6	0.7
33	2	20	0.25	13	13	1.00	0.6	0.7
34	3	15	0.50	7	12	0.90	0.6	0.7
35	4	12	0.25	8	15	0.75	0.6	0.7
36	2	20	0.10	17	17	1.00	0.6	0.7
37	2	20	0.50	7	9	0.75	0.7	0.8
38	4	12	0.25	9	13	0.90	0.7	0.8
39	4	12	0.25	5	10	0.10	0.7	0.8
40	2	20	0.50	7	12	0.90	0.7	0.8
41	2	20	0.50	6	9	0.50	0.7	0.8
42	3	15	0.10	13	13	1.00	0.8	0.9
43	5	10	0.25	7	9	0.25	0.8	0.9
44	2	20	0.50	3	15	0.50	0.8	0.9
45	3	15	0.50	4	12	0.50	0.8	0.9
46	2	20	0.50	4	12	0.50	0.8	0.9
47	3	15	0.75	4	15	0.90	0.8	0.9
48	5	10	0.10	9	13	0.90	0.9	1.0
49	6	11	0.25	4	7	0.10	$\boldsymbol{a} \succ_{\text{CRRA}} \boldsymbol{b}$	
50	6	12	0.75	4	7	0.50	$\boldsymbol{a} \succ_{\text{CRRA}} \boldsymbol{b}$	
51	6	11	0.50	3	8	0.25	$\boldsymbol{a} \succ_{\text{CRRA}} \boldsymbol{b}$	
52	6	11	0.75	3	8	0.50	$\boldsymbol{a} \succ_{\text{CRRA},\, \mu-\sigma} \boldsymbol{b}$	
53	6	8	0.50	3	5	0.50	$\boldsymbol{a} \succ_{\text{st}} \boldsymbol{b}$	
54	6	9	0.75	4	9	0.75	$\boldsymbol{a} \succ_{\text{st}} \boldsymbol{b}$	
55	5	9	0.50	4	8	0.75	$\boldsymbol{a} \succ_{\text{st}} \boldsymbol{b}$	
56	5	5	1.00	3	5	0.75	$\boldsymbol{a} \succ_{\text{st}} \boldsymbol{b}$	
57	5	7	0.50	4	6	0.50	$\boldsymbol{a} \succ_{\text{st}} \boldsymbol{b}$	
58	4	6	0.25	3	5	0.25	$\boldsymbol{a} \succ_{\text{st}} \boldsymbol{b}$	
59	5	8	0.10	5	8	0.90	$\boldsymbol{a} \succ_{\text{st}} \boldsymbol{b}$	
60	5	7	0.50	4	6	0.75	$\boldsymbol{a} \succ_{\text{st}} \boldsymbol{b}$	

Table 2.2: The 60 lottery pairs applied in the two conditions.
$\boldsymbol{a} \succ_{\text{st}} \boldsymbol{b}$ denotes 1st-order stochastic dominance of $\boldsymbol{a}$ over $\boldsymbol{b}$.
$\boldsymbol{a} \succ_{\text{CRRA}} \boldsymbol{b}$ indicates that $\boldsymbol{a} \succ \boldsymbol{b}$ under CRRA preferences for any ρ.
$\boldsymbol{a} \succ_{\mu-\sigma} \boldsymbol{b}$ indicates that $\boldsymbol{a}$ has both a higher mean and a lower variance than $\boldsymbol{b}$, so that $\boldsymbol{a} \succ \boldsymbol{b}$ for arbitrary mean–variance preferences.

Number of trials and conditions. There were two main conditions,

- no cognitive load ("$\mathrm{CL}_{\mathrm{no}}$"), i.e., no cognitive-load task at all;
- cognitive load ("$\mathrm{CL}_{\mathrm{yes}}$"), i.e., three sample points to remember.

Each condition comprised 60 trials. These trials were presented in blocks of 15 trials in pseudo-random order. All trials within a block belonged to the same condition. This was done to minimize carry-over effects between conditions.[6] Between blocks, subjects could take a break for as much time as they liked.

The duration of a trial that included the working-memory task ($\mathrm{CL}_{\mathrm{yes}}$) was 17.75 sec (see Figure 2.2) and of a trial that did not include the working-memory task ($\mathrm{CL}_{\mathrm{no}}$) 12.25 sec (see Figure 2.2, with stages C1–C3 and C4 omitted). The entire experiment lasted approx. 45 min, including 30 learning trials.

"Working-memory task only" trials. Since we anticipated having to estimate the treatment effect pooled across all subjects (and not on the individual level), we considered it useful to have an independent individual measure of task difficulty at our disposal that could be included as a covariate in our regressions.

One natural measure of task difficulty would be how well a given subject did in the working-memory task when also the lottery choice task was present. This measure of task difficulty would be confounded, however, with subjects' decisions how much attention to pay to the working-memory task relative to the lottery choice task. Imagine two in terms of cognitive ability identical subjects; one subject decides to attempt to be accurate in the lottery choice task, while the other one chooses to be rather accurate in the working memory task. This could have the effect that the subject with the *better* performance in the working-memory task exhibited *greater* risk aversion—which, given our hypotheses on the influence of cognitive load on risk aversion, is in exactly the opposite direction of what we would expect from a measure of task difficulty.

Hence, an *independent* measure of task difficulty is needed. This measure is provided by assessing subjects' performance in "working-memory task only" trials. We, therefore, included 30 trials in which subjects were given only the working-memory task, without having to make a lottery choice.

Learning trials. Subjects could familiarize themselves with the experimental design over the course of 30 learning trials. The first 10 learning trials consisted of the working-memory task alone, and the next 10 of the lottery choice task alone. In the last 10 learning trials, both tasks were being combined.

Remuneration. Subjects received remuneration for the lottery choice based on a randomly selected trial (see Section 2.3.1). The payoff was determined by randomly drawing a realization out of the lottery which the subject had chosen in that randomly selected trial.

[6] Imagine, e.g., being in a no-load trial after a high-load trial: It might well be that the increased cognitive load from the previous trial still has its repercussions in the current, no-load trial.

In addition to the lottery payoff, subjects received a reward of €5 upon answering correctly in the working-memory task. For correct measurement of the risk attitude, it is crucial that the reward for the working-memory task and the payoff stemming from the lottery choice are *independent of each other*. Therefore, one trial was selected randomly per subject for which the subject received a reward if and only if (s)he had answered correctly in the working-memory task.

Finally, subjects' remuneration included a show-up fee of €5.

Subjects. We tested $n = 41$ subjects (20 male, 21 female; age: range, 19–47 yrs.; mean ± std. dev., 25.9 ± 5.95 yrs.). Subjects were recruited mainly among the students of the Berlin universities and via mailing lists to which previous and prospective subjects had registered. Thus, the majority of subjects (30 of 41) were students from various disciplines; the occupational backgrounds of the remaining subjects ranged from electricians over university employees to physicians.

2.3.3 Additional measures of individual differences

In order to acquire some background knowledge on subjects, we administered the Cognitive Reflection Test (Frederick, 2005), and asked subjects to fill in a questionnaire (both after the experiment). In the questionnaire, subjects were asked to self-assess their willingness to take risks "in general" and in various domains, such as when driving a car, making financial decisions, or putting faith in other people. These questions were taken from the questionnaire[7] (p. 30) used in the 2004 wave of the German Socio-Economic Panel (SOEP).

Our questionnaire also asked subjects to make choices within two hypothetical settings with higher-stake lotteries. The first was again taken from the 2004 SOEP questionnaire (p. 30). The second asked subjects to determine a monetary amount $x \in [100, 300]$ that makes them indifferent between participation and non-participation in the lottery that pays −€100 or +€x with a probability of 50% each (i.e., a larger x indicates a higher degree of risk or loss aversion).

2.4 Results

2.4.1 Introductory remarks

To determine how our experimental manipulation influenced behavior, we compare, in a first step, the lotteries chosen by subjects in the cognitive-load condition to those chosen in the no-load condition (Section 2.4.4). We then

[7] The SOEP questionnaire is available at http://www.diw.de/documents/dokumentenarchiv/17/diw_01.c.40965.de/personen_2004.pdf.

show through structural regressions that increased cognitive load was accompanied by a significantly higher degree of relative risk aversion (Section 2.4.5). Finally, we show that across conditions, choices of the less risky lottery included in a lottery pair were, on average, made significantly faster than choices of the riskier alternative (Section 2.4.6).

2.4.2 Were the tasks adequate?

In only 5 of 4,920 ($41 \cdot 120$) lottery choices, subjects did not respond on time (0.102% of trials; 5 different subjects, four subjects during the no-load condition, one during the cognitive-load condition). There was not a single missed answer in the working-memory task ($41 \cdot 90 = 3690$ trials).

Taken together with the average response times reported in Section 2.4.6 as well as the hit rates reported in Section 2.4.3, these observations indicate that the tasks and permitted response times were adequately chosen.

2.4.3 How did subjects allocate attention to the two simultaneous tasks?

The average hit rate in the working-memory task decreased from 91.30% in the condition in which the lottery choice task was absent to 78.94% when the lottery choice task was present. Such a decrease could also be observed on the individual level for all subjects but two (see Figure 2.3). Thus, subjects see not to have focused exclusively on the working-memory task, but also paid attention to the lottery choice task. This is confirmed by the results presented in Section 2.4.4 and 2.4.5.

At the same time, it was also the case that the hit rates of all subjects were above chance level (50%) in the working-memory task even in the presence of the lottery choice task (see Figure 2.3). This is significant for all subjects but one on the 5% level, and significant for all subjects on the 10% level. Hence, the incentive (€5) to pay attention to the working-memory task also seems to have been adequate: Subjects did not pay attention exclusively to the lottery choice task.

	CL_{no}	CL_{yes}	*Marginal distribution*
Lower-variance lottery chosen	1454	1514	*2968*
Higher-variance lottery chosen	961	904	*1865*
Marginal distribution	*2415*	*2418*	*4833*

Table 2.3: Frequencies at which the lottery types were chosen (all subjects pooled).

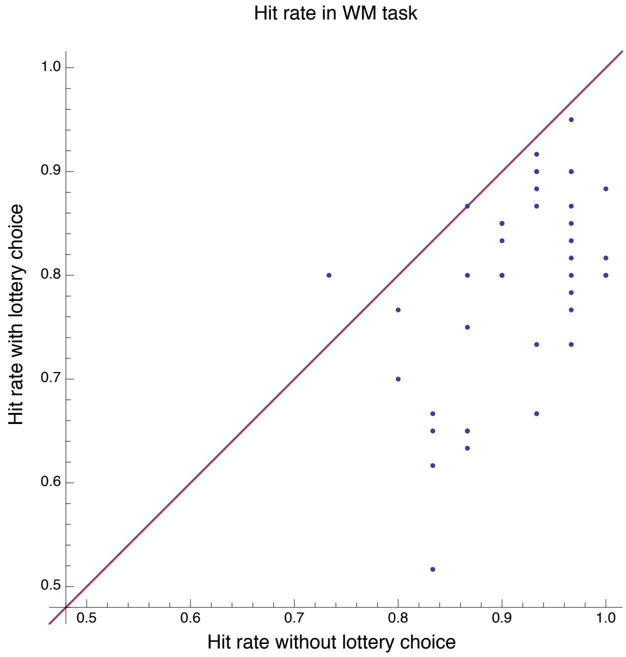

Figure 2.3: Influence of the presence of the lottery choice task on the percentage of correct responses in the working-memory task.

2.4.4 Preference reversal?—How often did subjects choose the riskier lottery?

Since each lottery pair was offered to each subject twice—once in the no-load and once in the load condition—we are able to check whether the experimental manipulation lead to choice reversals. As stated in Section 2.3.2, we expected choice reversals to be of the type that if under no cognitive load the riskier lottery is chosen from a lottery pair, the less risky option is chosen under increased cognitive load.

However, given that subjects do not make perfectly consistent choices, we expected such switches to occur also in the opposite direction. To determine whether the experimental manipulation has a *systematic* effect—so that one can indeed speak of a preference reversal—we need to check whether the number of switches in the one direction is significantly larger than the number of switches in the opposite direction (i.e., test whether the switches in the hypothesized direction account for a fraction significantly larger than 50% via a binomial test).

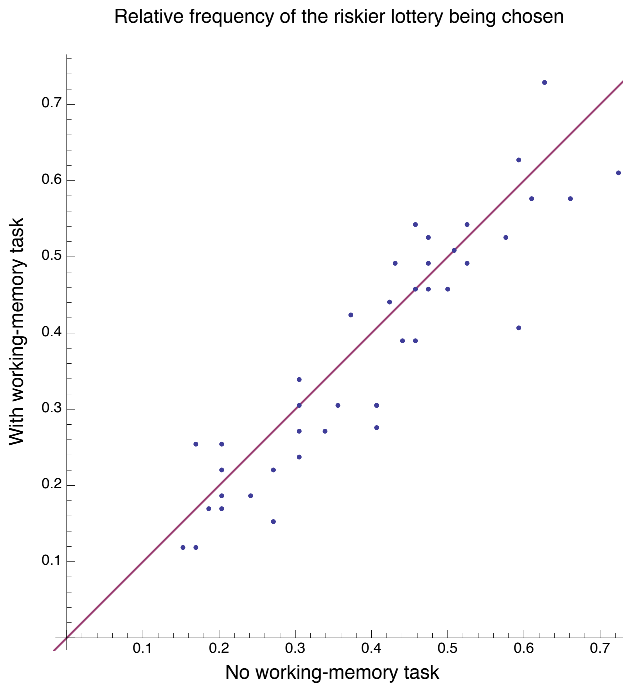

Figure 2.4: Influence of the presence of the working-memory task on the frequency at which subjects chose the riskier lottery.

Consistent with our hypothesis, the frequency at which the riskier lottery was chosen, was lower in the "high cognitive load" condition (see Table 2.3)[8]. The fact that the majority of points lies below the 45° line in Figure 2.4 reveals that the aggregate reduction in the frequency of "riskier" choices is not the result of a small number of subjects exhibiting a rather strong effect, but of a robust small effect across subjects.

A χ^2-test of Table 2.3 yields that the p-value of subjects' choices in the no-load condition and in the load condition stemming from the same distribution is 0.086.

If we restrict our attention to those lottery pairs for which a choice reversal was present at all, we can count how many of those choice reversals were indicating a switch away from the riskier lottery and how many were indicating a switch toward the riskier lottery under cognitive load.

Table 2.4 reports the respective numbers: Out of all 576 choice reversals, 317 were in the direction of choosing the higher-variance lottery in the no-load condition and the lower-variance lottery in the load condition, whereas only

[8] The total number of choices reported in Table 2.3 (4,833) is lower than the total number of choices made in the entire experiment (4,915) because, among others, the catch trials are not represented in this table.

	Change between no-load and load condition	
Lower-variance lottery chosen in load condition	317	Difference: 58
Higher-variance lottery chosen in load condition	259	
Total number of changes	*576*	

Table 2.4: Changes in the choice of the lottery with the higher/lower variance (all subjects pooled).

259 were in the opposite direction.[9] The p-value that 317/576 choices stem from a binomial distribution with $\pi = 1/2$ is only 0.00179, using a binomial test. Thus, there are significantly more switches from the higher-variance lottery to the lower-variance lottery when additional cognitive load is applied than in the opposite direction.

Is it possible that this choice pattern is the outcome of an increased number of random choices under cognitive load? Franco-Watkins et al. (2006, 2010) claim that Hinson et al. (2003) misinterpret their data—which supposedly demonstrate an influence of cognitive load on temporal discounting—in exactly this fashion.[10] Fortunately, in our experiment, we can rule out this potential confound. The reason is that in our case, the relative choice frequencies for the risky and less risky lottery do not approach 50% under cognitive load but get even more extreme: The less risky lottery is chosen in the majority of cases already in the absence of cognitive load, and it is chosen even more frequently in its presence. Thus, the pattern we observe cannot be explained through a tendency to choose more randomly under cognitive load.

[9] Note that the difference (317 − 259 = 58) reported here would have had to be identical to the difference between the number of riskier and the number of less risky choices (961 − 904 = 57) in Table 2.3, *if all subjects had made a choice in all trials.* Since, however, five subjects had one missed choice each, these two numbers may differ by up to 5.

[10] Franco-Watkins et al. (2006, 2010) argue along the following line: In the Hinson et al. (2003) experiment, participants had to choose between a smaller–sooner and a larger–later payoff. Subjects chose the larger–later payoff in substantially more than 50% of cases when not under cognitive load and in closer to 50% of cases under cognitive load. Crucially, random choice between the larger–later and smaller–sooner payoff would also lead to choice probabilities of around 50% for each of the two options. Thus, without additional information, one cannot decide whether an actual preference change—i.e., steeper discounting—or mere random choice was the reason behind Hinson et al.'s finding that the larger–later payoff was picked less often under load than without load.

2.4.5 Structural regressions: the influence of additional cognitive load on subjects' degree of relative risk aversion

Motivation

Checking for choice reversals uses a rather limited information set, since it does not take into account how similar or dissimilar in terms of subjective valuation the lotteries are for which the reversals occur. Using *all* choices that subjects made, we can detect whether the cognitive-load manipulation has an effect on decision making under risk by estimating the coefficient of relative risk aversion, ρ.

This is a standard approach in experimental economics (see, e.g., the review by Harrison and Rutström, 2008). It should, however, be noted that while using more information than merely counting choice reversals, it also rests on the rather strong assumptions that subjects act as expected-utility maximizers and that their choices can be well explained by a utility function with constant relative risk aversion (CRRA), i.e.,

$$u(x;\rho) \equiv \begin{cases} \dfrac{x^{1-\rho}-1}{1-\rho} & \text{if } \rho \neq 1 \\ \ln x & \text{if } \rho = 1 \end{cases}.$$

Estimation strategy

We have conducted four analyses to estimate the effect of the cognitive-load manipulation on the degree of relative risk aversion.

Common to all four regressions were the parameters that we estimated and the use of a probit link function:[11]

- ρ: the coefficient of relative risk aversion in the no-load condition,
- δ_ρ: the change of the coefficient of relative risk aversion due to presence of the simultaneous working-memory task,
- σ: the standard deviation of the link function—often called the Fechner noise parameter (see Harrison and Rutström, 2008, p. 76)—in the no-load condition,
- δ_σ: the change of the Fechner noise parameter due to the presence of the working-memory task.

The estimation proceeds as follows: For each lottery $\boldsymbol{l} \equiv (x_{\boldsymbol{l},1}, p_{\boldsymbol{l},1}; x_{\boldsymbol{l},2}, 1-p_{\boldsymbol{l},1})$, the expected utility $\mathrm{EU}(\boldsymbol{l};\rho)$ is calculated, using the utility function of the CRRA type mentioned above:

$$\mathrm{EU}(\boldsymbol{l};\rho) \equiv p_{\boldsymbol{l},1}\frac{x_{\boldsymbol{l},1}^{1-\rho}-1}{1-\rho} + (1-p_{\boldsymbol{l},1})\frac{x_{\boldsymbol{l},2}^{1-\rho}-1}{1-\rho}.$$

[11] Using a logit instead of a probit model leads to negligible changes.

Based on this, for each lottery pair $(\boldsymbol{a}, \boldsymbol{b})$, the difference

$$\Delta\mathrm{EU}(\boldsymbol{a}, \boldsymbol{b}; \rho) \equiv \mathrm{EU}(\boldsymbol{a}; \rho) - \mathrm{EU}(\boldsymbol{b}; \rho)$$

is determined. A rational decision maker with a coefficient of relative risk aversion equal to ρ would pick $\boldsymbol{a}$ over $\boldsymbol{b}$ if $\Delta\mathrm{EU}(\boldsymbol{a}, \boldsymbol{b}; \rho) > 0$ and $\boldsymbol{b}$ over $\boldsymbol{a}$ if $\Delta\mathrm{EU}(\boldsymbol{a}, \boldsymbol{b}; \rho) < 0$.

Based on this utility calculus, the regression tries to find values for ρ, δ_ρ, σ, and δ_σ so that the choices predicted by the model correspond as closely as possible to the binary choices that subjects actually made. More specifically, the objective is the following: Since subjects do not make choices that are perfectly consistent with the assumed model, one cannot expect to find a single pair of values of (ρ, δ_ρ) that explains all choices at once. Thus, one has to use a probabilistic estimation procedure. The most commonly binary-choice regressions are the logit and probit specification. They have in common that they map the difference of the expected-utility indices, $\Delta\mathrm{EU}(\boldsymbol{a}, \boldsymbol{b}; \rho)$, to choice probabilities via a (strictly increasing) link function, $f : (-\infty, +\infty) \to (0, 1)$, with $f(0) = \frac{1}{2}$. That is,

$$\begin{aligned}
\Pr[\boldsymbol{a} \mid (\boldsymbol{a}, \boldsymbol{b}); \rho, \sigma] &= f\left[\frac{\Delta\mathrm{EU}(\boldsymbol{a}, \boldsymbol{b}; \rho)}{\sigma}\right] \qquad \text{and} \\
\Pr[\boldsymbol{b} \mid (\boldsymbol{a}, \boldsymbol{b}); \rho, \sigma] &= 1 - \Pr[\boldsymbol{a} \mid (\boldsymbol{a}, \boldsymbol{b}); \rho, \sigma] = 1 - f\left[\frac{\Delta\mathrm{EU}(\boldsymbol{a}, \boldsymbol{b}; \rho)}{\sigma}\right].
\end{aligned}$$

The Fecher noise parameter σ in the denominator governs the dispersion (flatness) of the link function. The larger σ (i.e., the more noise), the smaller the fraction gets, with the effect that $\sigma \to \infty$ is equivalent to random choice. Conversely, $\sigma \to 0$ means that no noise is present at all from the perspective of the model, such that $\sigma \to 0$ indicates that choices are fully consistent with expected-utility maximization under CRRA preferences.

Let $\boldsymbol{c}_t$ denote the lottery that was actually chosen in trial t, and let $\mathbb{1}$ be the indicator function such that $\mathbb{1}[\boldsymbol{c}_t = \boldsymbol{a}_t] \equiv 1$ if $\boldsymbol{a}_t$ was chosen and 0 if $\boldsymbol{b}_t$ was chosen. Let $D_{\mathrm{CL},t}$ be a dummy regressor that equals 1 in trials t belonging to the cognitive-load condition and 0 otherwise. T is the total number of trials in the experiment.

In the case of non-linear least squares estimation, the regression minimizes the cumulative squared distance between the predicted choice probabilities and the actual choices:

$$\begin{aligned}
&\min_{\rho, \delta_\rho, \sigma, \delta_\sigma} \sum_{t=1}^{T} \left\{\Pr[\boldsymbol{a}_t \mid (\boldsymbol{a}_t, \boldsymbol{b}_t); \rho + \delta_\rho D_{\mathrm{CL},t}, \sigma + \delta_\sigma D_{\mathrm{CL},t}] - \mathbb{1}[\boldsymbol{c}_t = \boldsymbol{a}_t]\right\}^2 \\
= &\min_{\rho, \delta_\rho, \sigma, \delta_\sigma} \sum_{t=1}^{T} \left\{ f\left[\frac{\Delta\mathrm{EU}(\boldsymbol{a}_t, \boldsymbol{b}_t; \rho + \delta_\rho D_{\mathrm{CL},t})}{\sigma + \delta_\sigma D_{\mathrm{CL},t}}\right] - \mathbb{1}[\boldsymbol{c}_t = \boldsymbol{a}_t]\right\}^2. \qquad (2.1)
\end{aligned}$$

In the case of non-linear maximum likelihood estimation, the regression maximizes the log-likelihood

$$\ell(\rho,\delta_\rho,\sigma,\delta_\sigma) \equiv \sum_{t=1}^{T} \Bigg\{ \quad \mathbb{1}_{\boldsymbol{a}_t}[\boldsymbol{c}_t] \quad \ln f\left[\frac{\Delta\mathrm{EU}(\boldsymbol{a}_t,\boldsymbol{b}_t;\rho+\delta_\rho\, D_{\mathrm{CL},t})}{\sigma+\delta_\sigma\, D_{\mathrm{CL}}}\right] + \\ \{1-\mathbb{1}_{\boldsymbol{a}_t}[\boldsymbol{c}_t]\}\ln\left\{1-f\left[\frac{\Delta\mathrm{EU}(\boldsymbol{a}_t,\boldsymbol{b}_t;\rho+\delta_\rho\, D_{\mathrm{CL},t})}{\sigma+\delta_\sigma\, D_{\mathrm{CL}}}\right]\right\}\Bigg\}. \quad (2.2)$$

In the case of the probit function, the link function, f, is the cumulative density function of the standard normal distribution, $f[\Delta\mathrm{EU}] \equiv \Phi[\Delta\mathrm{EU}]$, while it is the logistic function, $f[\Delta\mathrm{EU}] \equiv 1/[1-\mathrm{e}^{-\Delta\mathrm{EU}}]$, in the case of the logit specification.

Regression 1: All subjects pooled, not controlling for individual heterogeneity, non-linear least-squares estimation

Regression 1 pooled the observations of all 41 subjects (i.e., 4195 choices). We used a probit model to estimate the four parameters mentioned above via non-linear least squares, see (2.1). The following results were obtained via MATLAB's `nlinfit` function:

$$\hat{\rho} = 0.6699, \quad \hat{\delta}_\rho = 0.0748, \quad \hat{\sigma} = 0.4505, \quad \text{and} \quad \hat{\delta}_\sigma = -0.0760.$$

All estimates are similar to values obtained in previous studies (see the overview in Harrison and Rutström, 2008). The parameter that we are most interested in, is $\hat{\delta}_\rho$. Its 95% confidence interval spans $[0.0078, 0.1419]$, so that we can conclude that δ_ρ is significantly larger than zero.

We had expected that the need to distribute attention over two simultaneous tasks in the presence of the working-memory task would lead to *less* consistency in subjects' lottery choices, i.e., $\delta_\sigma > 0$. Surprisingly, the opposite seems to be true, since $\hat{\delta}_\sigma = -0.0760 < 0$ (albeit not significantly so on the 5% level).

An explanation of $\hat{\delta}_\sigma < 0$ compatible with the multiple-systems approach is that under cognitive load, subjects rely more exclusively on the "emotional" evaluation process, which by itself leads to risk-averse but relatively consistent choices. Inconsistency in choices, according to this explanation, arises less due to inconsistent decisions made "within-system" but rather through interference "between-systems" when emotional and cognitive system compete with each other.

Coefficient	Regression 1 (NLLS) Estimate	p-value	Regression 2 (NLML) Estimate	p-value	Regression 3 (NLML, RE in ρ) Estimate	p-value
$\hat{\rho}$	0.6699	0.0000	0.6406	0.0000	0.6440	0.0000
$\hat{\delta}_\rho$	0.0748	0.0287	0.0720	0.0298	0.0673	0.0021
$\hat{\sigma}$	0.4505	0.0000	0.4444	0.0000	0.3089	0.0000
$\hat{\delta}_\sigma$	−0.0760	0.0721	−0.0505	0.3405	−0.0333	0.2257

Table 2.5: Results of the structural regression, all subjects pooled, without and with allowing for between-subject heterogeneity in ρ. "NLLS:" non-linear least-squares; "NLML:" non-linear maximum likelihood; "RE:" random effects.

Regression 2: All subjects pooled, not controlling for individual heterogeneity, non-linear maximum likelihood method

This regression is identical to Regression 1 apart from of the estimation method used: In this case we estimated the probit model via maximization of the log-likelihood function (2.2). The maximizers were found in an iterated grid search. The results differ only slightly from those obtained in Regression 1:

$$\hat{\rho} = 0.6406, \quad \hat{\delta}_\rho = 0.0720, \quad \hat{\sigma} = 0.4444, \quad \text{and} \quad \hat{\delta}_\sigma = -0.0505.$$

Again, the 95% confidence interval for $\hat{\delta}_\rho$, [0.0070, 0.1370], does not include 0, so that $\delta_\rho > 0$ is confirmed.

Regression 3: All subjects pooled, allowing for individual heterogeneity in ρ via random effects, maximum likelihood method

Given that individuals differ substantially in their attitudes toward risk (as is evident from the dispersion of the choice probabilities depicted in Figure 2.4), it is necessary to investigate whether allowing for individual heterogeneity in the regression analysis changes the conclusions. We, therefore, re-estimated the probit regression from Regression 1, this time allowing for individual heterogeneity in ρ via random effects. The estimation was done with MATLAB, using the `nlmefit`[12] function. The results are:

$$\hat{\rho} = 0.6440, \quad \hat{\delta}_\rho = 0.0673, \quad \hat{\sigma} = 0.3089, \quad \text{and} \quad \hat{\delta}_\sigma = -0.0333.$$

The 95% confidence interval for $\hat{\delta}_\rho$ is now $[0.0243, 0.1103]$, so that $\delta_\rho > 0$ is upheld. The fact that the estimate $\hat{\sigma}$ decreases substantially compared to the

[12] We checked that the estimation is robust w.r.t. different starting values. Especially, we tried negative starting values for δ_ρ. The algorithm converges to the reported significantly positive value $\hat{\delta}_\rho = 0.0673$ also for negative starting values.

Coefficient	Regression 4 Estimate	p-value
$\hat{\rho}$	0.6557	0.0000
$\hat{\delta}_\rho$	0.0879	0.0016
$\hat{\sigma}$	0.4465	0.0000
$\hat{\delta}_\sigma$	−0.0838	0.0000
Gender dummy (male = 1)	−0.2229	0.0000
Stated net income (in €100)	−0.0102	0.0020
Working-memory task performance (%)	−0.0203	0.0000
Presence of sure payoff (degenerate lottery)	0.4102	0.0000
Left/right dummy (larger payoff displayed left = 1)	−0.0622	0.0369
Subject's avg. RT in the lottery choice task (sec)	−0.0972	0.0002
Loss aversion question (−€100 vs. + €x)	0.0668	0.0000
Influence of gender on σ	0.2035	0.0000
Influence of presence of sure payoff on σ	−0.2816	0.0000

Table 2.6: Results of the structural regression, all subjects pooled, with controls for individual differences.

previous two regressions which did not account for between-subject heterogeneity regarding the degree of risk aversion makes sense: When not allowing for heterogeneity between-subjects, there are more unexplained/"inconsistent" choices, leading to a flatter link function, which is captured by an increase in the "Fechner error" estimate $\hat{\sigma}$ (see the explanation of the role of σ on p. 30).

Consequently, also when allowing for individual heterogeneity in the degree of risk aversion ρ, we conclude that the cognitive-load manipulation significantly increased subjects' degree of relative risk aversion.

Regression 4: All subjects pooled, controlling for individual heterogeneity in ρ and in σ through additional regressors, non-linear least-squares method

With this regression, we try to find out more about the factors which the observed between-subject variation in risk attitudes depends on. To do so, we included regressors for individual differences which previous studies found to correlate with risk attitudes. All these additional regressors were centered (i.e., they had mean zero). Hence, e.g., ρ is the average risk aversion in the no-load condition irrespective of gender.

The results are reported in Table 2.6. Most important, this regression also yields $\hat{\delta}_\rho = 0.0873 > 0$, with a p-value as low as 0.0019.

Concerning between-subject variation, we find (see Table 2.6)

- that female subjects were on average more risk-averse than male subjects (consistent with, e.g., Dohmen, Falk, Huffman, Sunde, Schupp, and Wagner, 2011b),
- that higher income goes along with reduced risk aversion (unlike previous studies which found no effect or even a positive effect, see Harrison and Rutström, 2008, Table 2), and
- that risk attitudes elicited experimentally with small stakes correlate with subjects' stated willingness to take risks in (hypothetical) larger-stake gambles, as indicated by the positive coefficient on x in the loss aversion question in Table 2.6 (see Section 2.3.3). This is consistent with Dohmen et al. (2011b).

Between-trials we find that a sure payoff seems to be especially attractive to subjects, which expresses itself in a higher estimated degree of risk aversion for those trials which featured a sure payoff. This is consistent with the Allais paradox and with the finding by Dickhaut, McCabe, Nagode, Rustichini, Smith, and Pardo (2003) that the presence of a sure payoff qualitatively changes subjects' brain activation compared to a situation in which both alternatives are non-degenerate lotteries.

Collectively, the mentioned findings indicate that our subject pool showed behavior that has also been observed in previous studies. This suggests that our results should be generalizable beyond the subject pool that we investigated.

We found no significant influence of age or performance in Frederick's (2005) Cognitive Reflection Test on risk aversion and excluded them as explanatory variables from the regression.

In addition to that we find that better performance in the working-memory task goes along with less risk aversion. This is in line with the research mentioned above (Section 2.2.2) that found performance in such tasks to be highly correlated with general cognitive ability (Evans, 2008, p. 259) in conjunction with the results of several studies finding that higher cognitive ability is on average accompanied by less risk aversion (see Section 2.2.5). We, furthermore, find that subjects who respond more slowly in the lottery choice task tend to be less risk-averse (which is in line with what Rubinstein, 2007, observed). Both observations are compatible with the multiple-systems hypothesis.

2.4.6 Analysis of the response times

Response times in the working-memory task

Figure 2.5 plots for each subject the average response time in the working-memory task in the absence vs. presence of the lottery choice task. The fact that the points lie above the 45° line for virtually all subjects indicates that having to engage in the lottery choice simultaneously increased response times. This

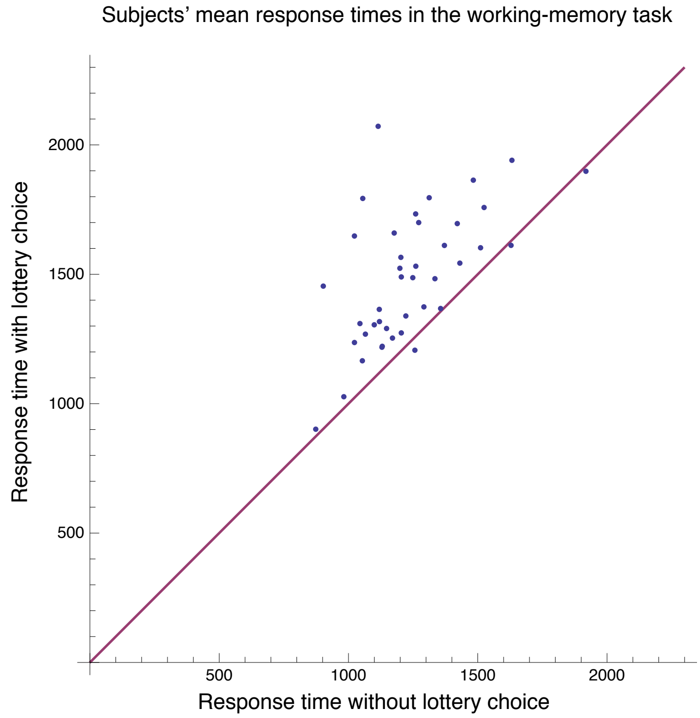

Figure 2.5: Influence of the presence of the lottery choice on the response times in the working-memory task.

goes along with a reduction in the number of correct responses, as was reported in Section 2.4.3 (Figure 2.3).

This could be due to two reasons: First, a change in this direction might be the result of the fact that the presence of the lottery choice increases the delay between the "sample points" and the "probe" phase, making it harder to keep the configuration of the sample points in the working memory. In our experiment, the delay between the "sample points" and the "probe" phase is as short as 1,800 ms in the absence of the lottery choice, while it is a full 9,800 ms in the presence of the lottery choice. Second, it could result from the multi-tasking demands in the presence of the lottery choice task, because multi-tasking usually has a negative impact on performance in each task involved, compared to the tasks being executed separately (see, e.g., Pashler, 1994).

Response times in the lottery choice task

Interestingly, the picture looks completely different with regard to the response times in the lottery choice. Figure 2.6 plots for each subject the average response time in the lottery choice task in the absence vs. presence of the working-memory task. The fact that the points lie below the 45° line for the vast majority

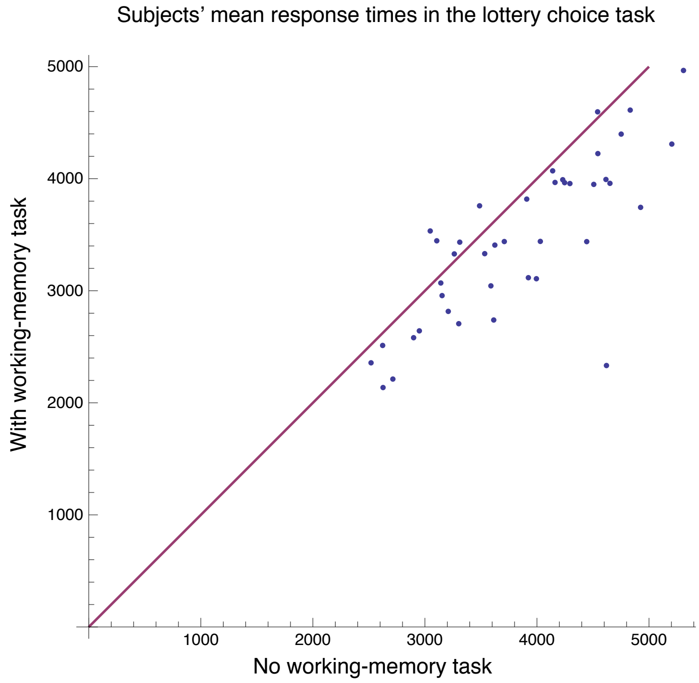

Figure 2.6: Influence of the presence of the working-memory task on the response times in the lottery choice.

of subjects indicates that subjects responded *more quickly* in the lottery choice task when having to execute the working-memory task simultaneously.

This finding is a little surprising, because one might have hypothesized that the multi-tasking demands of the cognitive-load condition lead to an increase in the time needed to arrive at a decision in the lottery choice task. At the same time, it is not completely surprising when considering the previous finding by Rubinstein (2007) that his subjects made decisions to choose the less risky of two options more quickly than decisions to choose the riskier of two options. Since we found risk aversion to increase with additional cognitive load, the observed decrease in lottery choice response times in the cognitive-load condition is consistent with Rubinstein's findings and with a dual-process explanation.

As we already argued in Section 2.2.5, Rubinstein's (2007) explanation that the response times he observes reflect the use of a "cognitive" or "instinctive" decision-making process is not the only possible explanation of the data he acquired, owing to the fact that he used a between-subject design.

Since we have data on 60 choices per condition per subject (apart from those five trials in which subjects did not respond), we can improve on Rubinstein's (2007) analysis by also estimating *within-subject* differences in response times

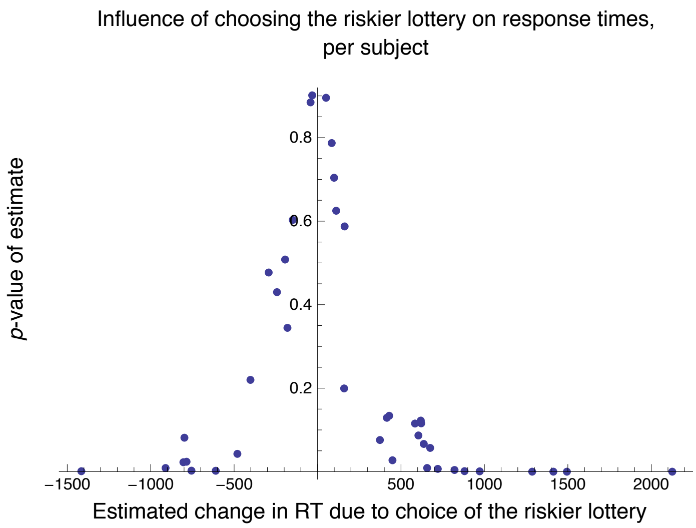

Figure 2.7: Per-subject estimates $\beta_{2,s}$ (i.e., change in average response time due to choice of the riskier lottery) and associated *p*-values.

between choice of the riskier and the less risky lottery. To do so, we estimated the parameters of the following equation separately for each subject s:

$$\mathrm{RT}_{s,t} = \beta_{1,s} + \beta_{2,s}\,D_{\mathrm{RLC},s,t} + \beta_{3,s}\,D_{\mathrm{CL},t} + \beta_{4,s}\,D_{\mathrm{SP},t} + \beta_{5,s}\,D_{\mu-\sigma,t} + \varepsilon_{s,t}, \qquad (2.3)$$

Here, $\mathrm{RT}_{s,t}$ denotes the response time of subject s in trial t; $D_{\mathrm{RLC},s,t}$ is a dummy regressor that takes on the value 1 if subject s chose the riskier lottery in trial t, and 0 otherwise; $D_{\mathrm{CL},t}$ is a dummy to control for a change in response times due to the presence of the working-memory task in trial t; $D_{\mathrm{SP},t}$ is a dummy that equals 1 in trials t in which a sure-payoff was present, and $D_{\mu-\sigma,t}$ is a dummy regressor that equals 1 if and only if trial t featured a lottery pair for which one lottery had a higher average pay-off, but also a higher variance than the second lottery. All four regressors were de-meaned, so that $\beta_{1,2}$ reports the average response time of subject s across all trials.

The latter two regressors are introduced as controls for the difficulty of trial t, with the hypotheses being that the presence of a sure payoff lead to shorter response times, and that trials in which a mean–variance tradeoff was present exhibited increased response times.

Probably the cleanest way to estimate the effect of "less risky" lottery choices is to carry out the regression for each subject individually. Least-squares estimation of Equation (2.3) for each subject individually yields the following across-subject average estimates

$$\bar{\hat{\beta}}_i \equiv \sum_{s=1}^{n} \frac{o_s}{\sum_{s=1}^{n} o_s}\,\hat{\beta}_s$$

[all values in ms], where o_s is the number of observations for subject s:

$$\overline{\hat{\beta}}_1 = 3646.9, \quad \overline{\hat{\beta}}_2 = 203.7, \quad \overline{\hat{\beta}}_3 = -375.8, \quad \overline{\hat{\beta}}_4 = -813.5, \quad \overline{\hat{\beta}}_5 = 431.8.$$

This means that the response time is on average 5.6% ($\overline{\hat{\beta}}_2 / \overline{\hat{\beta}}_1$) longer when the riskier alternative is chosen than when the less risky alternative is chosen, even after controlling for the effects of cognitive load and of the presence of a sure payoff.

There exists, of course, between-subject variation in the relation between choosing the riskier lottery and the response times, as it is captured by $\beta_{2,s}$. The range of estimates for the change in average response time due to choice of the riskier lottery is depicted in Figure 2.7. The figure reveals that the majority of estimates is positive ($\hat{\beta}_{2,s} > 0$ for 25 subjects vs. $\hat{\beta}_{2,s} < 0$ for 16 subjects), i.e., the positive average $\overline{\hat{\beta}}_2 = 203.7$ ms is not the consequence of a few extreme observations.

Figure 2.8 additionally shows that especially those estimates that are significantly different from 0 (i.e., that have a p-value < 0.05) are on average even substantially above 203.7 ms. This is something one might expect, but it isn't trivially true, because the p-value of an estimate is influenced not only by its magnitude but also by its standard error.

Pooled estimation of this equation without allowing for between-subject heterogeneity yields:

$$\hat{\beta}_1 = 3645.9, \quad \hat{\beta}_2 = 296.6, \quad \hat{\beta}_3 = -377.0, \quad \hat{\beta}_4 = -782.8, \quad \hat{\beta}_5 = 288.3.$$

All five estimates are significantly different from 0 (p-values < 0.0001).

If we allow for heterogeneity in this regression by introducing individual fixed effects in the average response time (β_1), we get:

$$\hat{\beta}_1 = 3645.9, \quad \hat{\beta}_2 = 212.1, \quad \hat{\beta}_3 = -380.0, \quad \hat{\beta}_4 = -801.4, \quad \hat{\beta}_5 = 324.0,$$

Comparing these results with the results of the pooled regression that does not not allow for between-subject heterogeneity, we observe that the coefficient on the change in response times due to choosing the riskier lottery ($\hat{\beta}_2$) decreased from 296.6 to 212.1 [ms]. In exchange, the coefficient on the presence of a mean–variance tradeoff ($\hat{\beta}_5$) increased from 288.3 to 324.0 [ms]. That is, more of the variance in response times is now explained in terms of task difficulty and less is attributed to choice of the riskier lottery. Nevertheless, all estimates are still significantly different from 0 (all p-values < 0.0001).

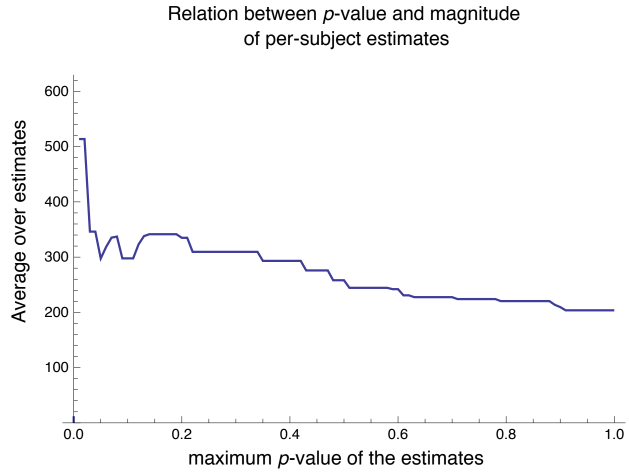

Figure 2.8: Per-subject estimates with low p-values point strongly in the direction of an increase in response times caused by choice of the riskier lottery.

Finally, if we model between-subject heterogeneity through individual random effects in the average response time (β_1), we get:

$$\hat{\beta}_1 = 3489.4, \quad \hat{\beta}_2 = 166.4, \quad \hat{\beta}_3 = -383.7, \quad \hat{\beta}_4 = -779.8, \quad \hat{\beta}_5 = 420.4,$$

These values are also all significantly different from 0 (all p-values < 0.001), even though $\hat{\beta}_2$ has shrunken further.

Taken together, we find robust evidence that it took subjects longer to choose the riskier lottery than to choose the less risky lottery. We are, thus, the first to provide this type of evidence via *within-subject* measures. The dependence of response times on the decision outcome indicates that choice under risk is not the product of a unitary decision-making process but of interacting processes (as argued in Section 2.2.5). Only now is Rubinstein's (2007) interpretation that risk aversion is, in part, the consequence of "instinctive reasoning" warranted.

2.5 Discussion

We are the first to show *within-subject* that additional cognitive load increases subjects' risk aversion. We do so by observing that subjects switched significantly more often from the riskier lottery to the less risky lottery when cognitive load was increased than in the opposite direction. We confirm this finding by computing several structural regressions to assess the change in the degree of relative risk aversion induced by the cognitive-load manipulation. Across four different specifications, all estimates of this parameter of interest turned out to be significantly positive.

This finding provides evidence that the multiple-systems approach to decision making can explain not only individual differences in risk attitudes but also within-subject variation in response to situational influences.

Importantly, this finding is corroborated by two findings on response times that point in the same direction. The first is that in the cognitive-load condition, response times in the lottery choice task were faster than in the no-load condition. This connection between risk-avoidance and comparatively fast responses has previously been reported by Rubinstein (2007).

Our second finding concerning response times is that even within-condition, a large majority of subjects responded faster on average when choosing the less risky than when choosing the risky lottery. Only based on this within-subject finding, Rubinstein's (2007) interpretation of his between-subject results that risk aversion is partially the consequence of "instinctive reasoning" is valid.

Could these results be explained also without reliance on a dual-system approach? We think they cannot:

1. Could the observation that risk-avoiding choices are made quicker than risk-accepting choices be the consequence of subjects applying a heuristic while making the decision?

   A heuristic that would specifically predict this pattern is the priority heuristic (Brandstätter et al., 2006): It assumes that subjects evaluate the offered lotteries component-wise, with the first step being that the minimum payoffs of the presented lotteries are compared. By assumption, only if these minimum payoffs differ by more than 10% of the difference between the maximum payoffs, the evaluation continues by taking the probabilities associated with the payoffs into account. Otherwise the evaluation stops and the lottery with the larger minimum payoff (which usually also features the lower maximum payoff) is chosen. Thus, risk-avoiding choices (choices of the lottery with the larger minimum payoff) are made quicker than risk-accepting choices, because for the latter to be able to occur, another step in the heuristic needs to be carried out (this second step need not but can lead to choice of the riskier lottery).

   Such a heuristic would not need to rely on two systems to generate the observed response time pattern. Two things that the priority heuristic, however, does not predict are (*a*) the observation that risk aversion is higher under cognitive load and (*b*) that choices are made faster under cognitive load than in the no-load condition. It can, thus, be excluded that the behavior we observe is solely produced by the priority heuristic.

   More generally, any heuristic that is no itself based on the dual-system hypothesis is bound to be unable to explain why lesions in certain brain areas, as found by Hsu et al. (2005) and Shiv et al. (2005), lead to diminished risk aversion.

2. Could it rather be that the cognitive-load manipulation changes the perceived riskiness of the presented lotteries than risk preferences?

   Such a change in the perceived riskiness would have to put the riskier lottery at a disadvantage under cognitive load to be compatible with our findings. For instance, it might be that subjects focussed less on the overall characteristics of the presented lotteries (say, their expected values) but more on their components (see the description of the priority heuristic above), and that they chose the lottery that maximizes the minimum possible outcome (a maxmin strategy like Gilboa and Schmeidler, 1989, suggest for decision making under ambiguity).

   If the latter was true, one would expect the cognitive-load manipulation to cause a rather dramatic increase in the measured degree of risk aversion, because the lottery with the higher minimum payoff would be chosen regardless of its further properties. This is not what we observe.

   What we do find, in contrast, is the following: We included a risk-free alternative in several of the lottery pairs. We found that the presence of a risk-free alternative increased the measured degree of risk aversion, see Regression 4 (Section 2.4.5). That is, we modeled the degree of relative risk aversion in trial t, ρ_t, to be influenced as follows:

   $$\rho_t \;=\; \rho + \delta_\rho\, D_{\mathrm{CL},t} + \delta_{\mathrm{SP}}\, D_{\mathrm{SP},t} + \ldots,$$

   where $D_{\mathrm{CL},t}$ and $D_{\mathrm{SP},t}$ are the already familiar respective dummy variables for the presence of the cognitive-load manipulation and the presence of a sure payoff in trial t.

   If the perceived relative riskiness was influenced by cognitive load to the disadvantage of the riskier option, we would expect the risk-free alternative to be especially attractive under cognitive load. That is, we would expect δ_{SP} to depend on the presence of greater cognitive load: ρ_t should include an interaction term as follows:

   $$\rho_t \;=\; \rho + \delta_\rho\, D_{\mathrm{CL},t} + \delta_{\mathrm{SP,\,dir}}\, D_{\mathrm{SP},t} + \delta_{\mathrm{SP,\,int}}\, D_{\mathrm{CL},t} \times D_{\mathrm{SP},t} + \ldots.$$

   The hypothesis would be that the coefficient on the interaction term is positive: $\delta_{\mathrm{SP,\,int}} > 0$.

   If we include the proposed interaction term in our regression, we find its coefficient to be slightly negative ($\hat{\delta}_{\mathrm{SP,\,int}} = -0.0110$) and insignificantly different from zero (p-value: 0.9326). That is, it does not seem to be the case that subjects favor the sure payoff under cognitive load more than without load. Thus, it is unlikely that the cognitive-load manipulation puts a burden on subjects which affects the evaluation of the riskier and the less risky lottery asymmetrically to the disadvantage of the riskier alternative.

Our within-subject findings, therefore, harden the evidence that cognitive load indeed induces a change in risk preferences. They confirm the results reported by Benjamin et al. (2006) and strengthen them in an important way. Furthermore, our within-subject findings on response times provide a basis for Rubinstein's (2007) claim that risk aversion is, in part, generated by "instinctive reasoning." Consequently, our findings affirm the idea advocated by Fudenberg and Levine (2006, 2010), to explore behavior under risk with the help of models which are inspired by the dual-system approach.

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3 Social learning in asset markets—a peek into the herding brain

Joint with David N. Danz, Guido P. Biele, Harald Uhlig, Dorothea Kübler, and Hauke R. Heekeren

Contributions: H. G., D. N. D., G. P. B., H. U., D. K., and H. R. H. designed research; H. G. and D. N. D. acquired data; H. G. and D. N. D. analyzed data; H. G., D. N. D., G. P. B., and H. U. wrote the text.

3.1 Introduction

When humans make decisions, they often take into account what others have decided before them. Think, for example, of investment decisions or school choice: one might survey colleagues and friends about which stocks they have invested in and which school they have sent their children to.

Learning from others—called "social learning"—has received considerable attention in the economics literature. In the "pure form" of social learning, players' actions generate only informational externalities, but no payoff externalities. Specifically, in the most basic theoretical model (Bikhchandani, Hirshleifer, and Welch, 1992), there exist several agents who all face the task of predicting the state of the world. The order in which these agents make their predictions is given exogenously. For simplicity, it is assumed that there are only two possible states of the world, A and B (e.g., either University A is better than University B or vice versa). Each agent j receives an informative, but noisy idiosyncratic signal s_j on the true state of the world (i.e., $s_j = a$ or $s_j = b$ with $p_j = \Pr[s_j = a|A] = \Pr[s_j = b|B] = p > 0.5$). Social learning is introduced through the assumption that agents can observe the string of all previous predictions by others (e.g., $ABAA$) in addition to their private signal. No payoff externalities means that each agent's payoff is determined by her own prediction alone—say, 1 if the agent's prediction was correct and 0 otherwise.

In this context, an *informational cascade* and *herding* occur when agents up from a certain position in the sequence do not take their idiosyncratic signals into account any more but base their own predictions solely on the predictions of their predecessors. Such herding behavior is *rational* if the informational content of the preceding decisions is higher than the informational content of one's own private signal. Under the assumption of rational agents, therefore, the theoretical prediction for this type of environment is that up from a certain length of the sequence, almost surely, people base their prediction solely on their predecessors' information and herd (rationally). They eventually all end up doing the same. In that case, social learning stops.[1]

Due to its simplicity, this setup lends itself readily to experimental investigation in the laboratory. Consequently, the theoretical predictions have been tested in a number of studies. The robust finding of these studies is that subjects learn *less* from their predecessors than theory implies (e.g., Nöth and Weber, 2003; Kübler and Weizsäcker, 2004; Goeree, Palfrey, Rogers, and McKelvey, 2007; Weizsäcker, 2010).

An explanation of this observation is that subjects suspect that preceding players made errors in their predictions, which diminishes the informational content of observing their choices. This is not captured by the standard theoretical model.

A recent model-free meta-analysis of social-learning experiments (Weizsäcker, 2010), however, casts doubt on the idea that this is the sole explanation of why subjects' behavior deviates from the theoretical predictions: the meta-analysis reveals that subjects would not only have had to assume that their predecessors made errors, but they would have had to substantially overestimate their predecessors' error rates.

This raises the question why exactly it is that people often do not learn from others when conventional theory predicts that they should. An alternative to the hypothesis that subjects overestimate preceding players' error rates is that they are averse against the uncertainty (ambiguity) of potentially being misled by (erroneous) choices of others. Thus, this alternative rather attributes the observed behavior to subjects' preferences than to their beliefs. Concerning preferences, there might simultaneously be a countervailing force that biases people in the direction of following preceding players in the form of a desire for "social conformity," as it is called in the psychological literature (see the review by Raafat, Chater, and Frith, 2009) and as it has also been found in an economic laboratory experiment (Goeree and Yariv, 2007).

In purely behavioral experiments, it is hard to disentangle these influences. Using neuroeconomic methods, however, might help to separate the contributions of the three forces. Therefore, we let subjects participate in a social-learning experiment while we recorded their brain activation via functional magnetic resonance imaging (fMRI).

We find evidence that subjects' decisions are not driven by beliefs about others' erroneous responses alone, but that preferences also play a role. As a matter of fact, however, the results of our analysis of subjects' response times

[1] With positive probability it does so in a situation where everyone makes the wrong prediction.

and their brain activation indicate that subjects need to overcome a tendency to follow the preceding player in situations where they choose to obey their idiosyncratic signal. Consequently, if anything, subjects should follow others too often, not too little. Our study, thus, makes the puzzle of too little social learning even more puzzling by providing additional evidence that a desire for social conformity also seems to be at work.

3.2 Experimental design

3.2.1 Introductory remarks

Our experimental design is based on the canonical herding experiment that mimics the setup of the model developed by Bikhchandani et al. (1992). We deviated from the canonical design in two ways:

1. We introduced a "computer condition" as a benchmark for comparison.
2. We let the signal qualities p_j differ across agents j.

The reasons for the latter change is explained below (Section 3.2.2).

We also kept the social-learning situation as simple as possible. This means that we only considered sequences of decision makers of length two, and we focus our analyses on the decision makers at position two.

3.2.2 Task

Conditions

The experiment consisted of 210 trials. Each trial belonged to one of two conditions: the "computer condition (CC)" or the "human condition (HC)." The computer condition comprised 60 trials (4 blocks à 15 trials), while the human condition comprised 150 trials (10 blocks à 15 trials). The 14 blocks were ordered as follow:

HC – HC – CC – HC – HC – CC – HC – HC – CC – HC – HC – CC – HC – HC.

Thus, the setup had features of both a block design and an event-related design. A trial lasted 14.5 sec on average, so that the entire experiment lasted around 50 min (net, without rest periods; 56 min including rest periods).

At the beginning of each block, subjects were informed on screen about the type of the respective block (see Figure 3.1, left panel, for the computer condition and the left panels of Figures 3.2 and 3.3 for the human condition).

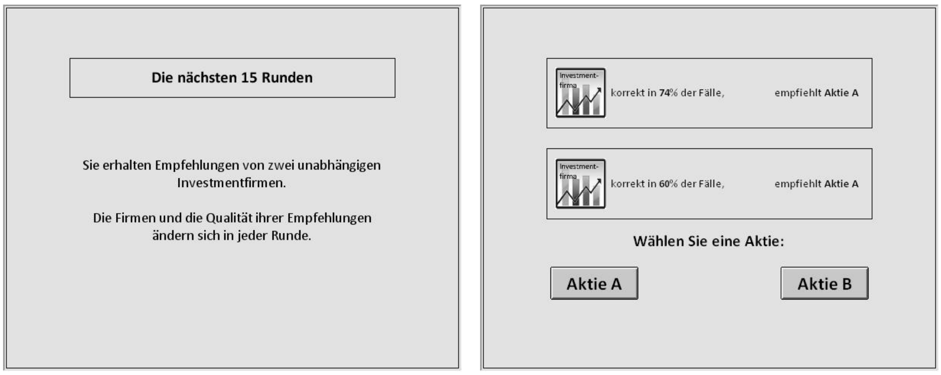

Figure 3.1: Hardcopies of the screens displayed in the computer condition. In the "decision" screen (pictured right), the second line of text, containing the information on the second firm's recommendation, was displayed on average 2.5 sec (jittered) after display of the first line of text.

Trial setup

In both the computer and the human condition, subjects had to decide which of two available "stocks," A or B, to "invest" in. Only one of the two assets paid off in a given trial. It was known to subjects that, *a priori*, in a given trial, each asset was equally likely to be the profitable one. Subjects did *not* receive feedback during the experiment whether they had chosen the profitable asset. (Only at the very end, when their payoffs were determined, subjects were informed about the profitability of their choices.)

In each trial subjects received two hints as to which of the two stocks was the profitable one in the current trial. What distinguished the two conditions was the nature of the two hints:

In the *computer condition*,[2] subjects received two signals that were independently drawn by a computer. Subjects were told that these signals represented purchase recommendations, obtained from "two independent investment firms." The probabilities ("signal qualities"), $p_{1,t}$ and $p_{2,t}$, that Firm 1 resp. Firm 2 correctly recommended the profitable asset varied from trial to trial. In each trial t it held that $p_{1,t} > p_{2,t}$. The first signal, $s_{1,t}$, as well as its quality $p_{1,t}$ were displayed first. After a short delay (2.5 sec, jittered), also the second signal, $s_{2,t}$, and the associated $p_{2,t}$ were displayed. Subjects were informed of $p_{1,t}$ and $p_{2,t}$ in each trial anew (see Figure 3.1).

The *human condition* differed from the computer condition in only one respect: information about the first signal was replaced by information about the investment decision of a human being (henceforth, "first player" or "preceding player"). First players had made their decisions after seeing the first, but not

[2] Of course, the terms "computer condition" and "human condition" were not used in the instructions given to subjects.

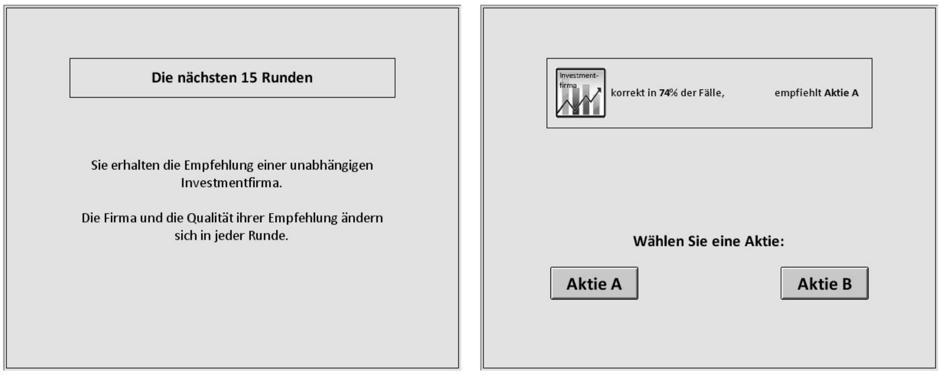

Figure 3.2: Hardcopies of the screens displayed to the *first players* in the human condition.

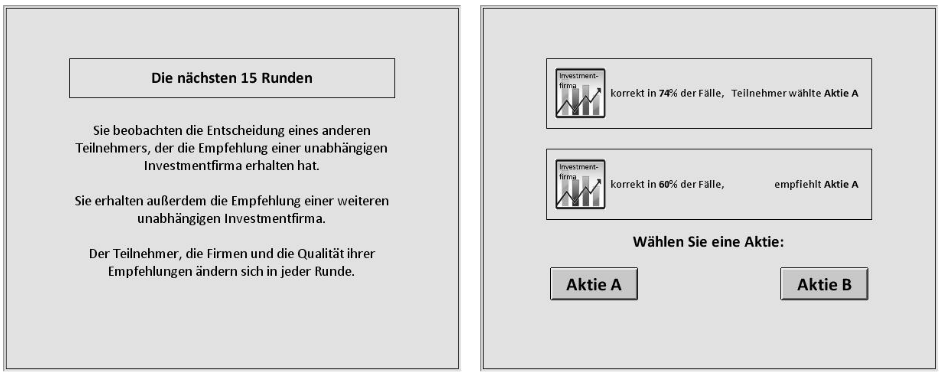

Figure 3.3: Hardcopies of the screens displayed to the *second players* in the human condition. In the "decision" screen (pictured right), initially only the first line of text, informing the subject of the first player's action, was being displayed. The second line of text, containing the information on the investment firm's recommendation, was displayed on average 2.5 sec (jittered) later.

the second signal. (The first players' "decision" screen is shown in Figure 3.2, pictured right.) Along with the first player's decision, subjects observed the quality $p_{1,t}$ of the first signal (see Figure 3.3). Just as in the computer condition, after a short delay (2.5 sec, jittered), the second player was shown the second signal, together with the signal quality $p_{2,t} < p_{1,t}$.

There was a pool of 16 first players. In each round we matched second players anew and randomly to a first player.

In both conditions, subjects had to make a decision within 6 sec after the onset of the display of the second hint. Subjects indicated their choice by pressing one of two buttons with a finger of their right hand. The chosen stock was surrounded by a red frame as soon as the subject had pressed a button. Within the time window for making a choice, subjects could change their decision.

All information presented to subjects while lying in the MRI scanner was displayed via video goggles. Between trials, there was an interval of approximately 6 seconds (jittered) during which a fixation cross was displayed on screen.

Choice of the signal qualities

As mentioned previously, we chose the combinations of signal qualities $(p_{1,t}, p_{2,t})$ such that always $p_{1,t} > p_{2,t}$. In doing so, we deviated from the basic model (Bikhchandani et al., 1992)—which maintains that $p_{1,t} = p_{2,t}$—and, thereby, from the canonical setup of economic herding experiments. We knew from the experimental literature cited before that with $p_{1,t} = p_{2,t}$, most second players would rely on their own signal, and no herding would take place. Since we are interested in what distinguishes herding from non-herding choices, we needed variability in subjects' decisions. Therefore, some gap between the signal qualities and, thus, some incentive to follow the first player was needed.

This, however, entailed the following: we introduced a potential confound in the comparison of behavior and brain activation between the computer and the human condition. Imagine the extreme-case scenario that in the computer condition, second players always chose the asset recommended by Firm 1 (as implied by maximization of expected payoff), and that in the human condition, they always chose the asset recommended by Firm 2. Since $p_{1,t} > p_{2,t}$, the expected reward in the computer condition would be higher than in the human condition, such that behavioral differences and differences in brain activation introduced by the distinctness of the two conditions would be confounded by the difference in reward probability.

Consequently, in our analyses, we had to control for reward probability. This need imposed some restrictions on the distribution of $(p_{1,t}, p_{2,t})$. In order not to generate additional confounds, we generated 180 $(p_{1,t}, p_{2,t})$ combinations with the following properties:

- The distributions of $p_{1,t}$ and $p_{2,t}$ overlapped to a large extent; more specifically, their supports were $[0.61, 0.89]$ and $[0.6, 0.87]$. The means were 0.77 for $p_{1,t}$ and 0.70 for $p_{2,t}$ (in both the computer condition and the human condition). Due to the overlap we can more confidently assume a linear model, while the rather wide support of the two distributions should enable us to uncover, if present, both behavioral changes and changes in the blood oxygenation level-dependent signal (the so-called fMRI BOLD signal) that depend on the signal qualities.
- The differences $\Delta p_t \equiv p_{1,t} - p_{2,t}$ were all within the interval $[0.01, 0.15]$ and were distributed close to uniformly on that interval.
- $(p_{1,t}, p_{2,t}) \neq (p_{1,\tau}, p_{2,\tau})$ for all $\tau \neq t$. This ensured that the entire support of the two distributions was filled by realizations; in addition, it also contributed to the following:

- $p_{1,t}$ and $p_{2,t}$ were only weakly correlated with $\Delta p_t = p_{1,t} - p_{2,t}$ (Corr[$p_{1,t}$, Δp_t] = 0.33 and Corr[$p_{2,t}, \Delta p_t$] = −0.30), so that we avoid multicollinearity in case we want to simultaneously include both Δp_t and $p_{1,t}$ or $p_{2,t}$ as regressors in an analysis of the behavioral or BOLD data. The fact that $p_{2,t}$ was displayed with a jittered delay after $p_{1,t}$ further reduces the correlation of the regressors for Δp_t and $p_{1,t}$, if included simultaneously in the BOLD analysis, because they have different temporal onsets.

The 180 ($p_{1,t}, p_{2,t}$) pairs thus generated were used in the 60 CC trials and in 120 of the 150 HC trials. In the remaining 30 trials of the human condition, we set ($p_{1,t}, p_{2,t}$) = (0.74, 0.69). The reason for the introduction of this "special pair" of probabilities was that we wanted to be able to detect potential "switching behavior" of subjects. We had observed such switching behavior during our behavioral pilot studies: Subjects who had decided perfectly rationally in the computer condition, switched between following Firm 2's recommendation and following the first player's action for one and the same ($p_{1,t}, p_{2,t}$) combination.[3] We chose the combination (0.74, 0.69), because switching behavior had been most pronounced for relatively small Δp in the pilot experiments.

Trials with identical and contrary hints

Table 3.1 provides an overview of the different decision situations which subjects faced during our experiment. "Identical hints" means that both signals (in the computer condition) recommended the same stock (i.e., $s_1 = s_2 = a$ or $s_1 = s_2 = b$) or that signal 2 recommended the same stock as the first player had chosen (i.e., $H_2 = H_1$) in the human condition. "Contrary hints" refers to the situation in which the two hints were not aligned ($H_1 \neq H_2$).

Given that the signals were informative, so that $p_{1,t}$ and $p_{2,t}$ exceeded $\frac{1}{2}$, the majority of trials would normally have featured signals that both recommended the same stock (i.e., "identical hints" trials). These trials, however, are the most uninteresting for the analysis of social learning, because in these trials social learning and obeying one's private signal are indistinguishable.

Moreover, scan time is costly, and subjects tend to get tired while lying in the MRI machine. We, therefore, chose not to extend the experiment's duration beyond 50 min (plus the time needed for acquiring the anatomical scan and rest periods). Since we still needed to gather enough behavioral and fMRI data for those trials that are of greater interest—i.e., the trials with contrary hints—we presented to subjects mainly trials with contrary hints, see Table 3.1.

[3] It is a challenge to explain this behavior, as it is neither compatible with expected-utility maximization nor with ambiguity aversion: under both types of preferences there is no motive for such "diversification" of behavior (unless subjects were truly indifferent between both actions). People may have pursued some kind of probability matching, but then it is unclear why they did so only in the human condition and not in the computer condition.

		Identical hints ($H_1 = H_2$)	Contrary hints ($H_1 \neq H_2$)	*Total: 210*
Computer condition		20	40	*60*
Human condition	(p_1, p_2) varying:	40	80	*120*
	$(p_1, p_2) = (74\%, 69\%)$:	4	26	*30*

Table 3.1: Number of decision situations per condition and per congruency of the hints.

Of course, we made sure that conditional on the trial type, the likelihood of receiving a reward from the two possible actions (obeying hint 1 or hint 2) was consistent with the signal qualities displayed on screen.[4] And of course, in line with the "no deception" policy in experimental economics, subjects were informed about this selection of the trials in the instructions.

Remuneration

Subjects received a show-up fee of €15 and a variable payment that depended on their performance: €1.30 for each choice of the profitable stock in 20 randomly selected trials. That is, they had a financial incentive to pick the stock that they thought was more likely to be profitable in each given trial.[5] This also holds for the first players whose choices our subjects observed.

3.2.3 Subjects

We scanned only second players. 35 healthy adult subjects participated in the experiment. One of them had to be excluded from all analyses due to falling asleep repeatedly and for prolonged periods in the scanner. Of the remaining 34 subjects, 16 were female and 18 were male (mean age ± standard deviation, 26.71 ± 3.14 years; range 22–32 years). All subjects were right-handed, had graduated from high school ("Abitur", "allgemeine Hochschulreife"), and had no known prior neurological or psychiatric disorder.

The study was conducted with the approval of the ethics committee of the Max Planck Institute for Human Development. Subjects received written instructions at the beginning of the experiment and gave informed consent to participate in the experiment. Before being admitted to the scanning phase,

[4] For example, in trials with contrary hints in the computer condition, the likelihood of picking the profitable stock when obeying signal 1 was slightly above 50%, while in trials with identical hints it was in the range of 80–90%.

[5] Note that, unlike in experiments designed to elicit risk attitudes that involve repeated choices between different lotteries, no problem arises in our experiment from paying out multiple trials. Since in the case of conflicting hints, only one hint can be correct, there is no possibility for subjects to diversify their risk by hedging across trials.

subjects had to correctly fill out a questionnaire that we designed to check that they had understood the experimental rules. To get acquainted with the permitted response time, subjects completed several learning trials before entering the MRI scanner.

3.2.4 fMRI acquisition

Subjects were placed in a light head restraint in the scanner to limit head movement over the course of the experiment.

Gradient echo $T_2^{\star}$-weighted echo-planar imaging (EPI) data with blood oxygenation level-dependent (BOLD) contrast were acquired on a Siemens Trio 3-Tesla full-body MRI scanner (Siemens Medical Systems) at Charité—Universitätsmedizin Berlin, Campus Benjamin Franklin. A head coil was used for radio frequency signal transmission and signal reception. The sequence enabled acquisition (interleaved) of 36 axial slices of 3 mm thickness and a voxel size of 3 mm × 3 mm × 3 mm at a repetition time [TR] of 2 s. Further imaging parameters were: time to echo [TE], 29 ms; field of view, 192 mm^2; flip angle α, 90°.

The functional imaging data were acquired in two separate runs with a duration of 840 volumes (840 volumes × 2 sec/volume = 1,680 s = 28 min) each. For each run, the first three images were discarded to allow for steady-state longitudinal magnetization to be obtained. Each run was concluded by 5 volumes, or 10 s, without stimuli.

After the two runs, a T_1-weighted high-resolution anatomical image (192 slices, voxel size: 1 mm × 1 mm × 1 mm, TR = 1900 ms, TE = 2.52 ms) was acquired for each subject.

3.2.5 Prior hypotheses

Interacting with other human beings requires that we understand what situation others are in, that we can take—at least to a certain degree—their point of view, and infer their intentions. This ability is often termed “mentalizing,” “perspective taking,” or “theory of mind.” Various tasks which induced experimental subjects to engage in mentalizing have been shown to activate specific brain areas like superior temporal sulcus (STS), temporo-parietal junction (TPJ), temporal poles and medial prefrontal cortex (mPFC) in a “remarkably consistent” way (Frith and Frith, 2006). For this reason, this set of brain areas has been designated the “mentalizing network.”

Importantly for our study, mentalizing-related brain areas have been shown to be also activated in economic settings that required subjects to interact with others (Coricelli and Nagel, 2009; Hampton, Bossaerts, and O’Doherty, 2008). The hypotheses that we formulated prior to running the experiment, therefore, stipulated that we should also observe increased activation in mentalizing-associated areas when comparing the human condition with the computer condition. Furthermore, we expected subjects' behavior—i.e., their propensity to follow preceding players—to covary with activation in mentalizing-associated brain areas. Specifically, we hypothesized the following:

H1 Mentalizing-associated areas like anterior cingulate cortex (ACC) and superior temporal sulcus (STS) show greater activation in the human condition than in the computer condition (within-subject contrast). In this contrast, we have to control for the reward probability (see the explanation in Section 3.2.2).

H2 In the human condition, mentalizing-associated areas show greater activation for intermediate differences $p_1 - p_2$, because in these cases the second movers have to deliberate harder on the correctness of the first movers' decision (within-subject contrast).

H3 Holding $p_1 - p_2$ constant, mentalizing-associated areas show greater activation for higher p_1 in the HC, because in these cases the expected payoff from a correct decision is higher, incentivizing subjects to deliberate harder on the probability that the first mover committed an error (within-subject contrast).

H4 Mentalizing-associated areas might show greater activation for those trials in the HC in which subjects do not follow the first movers' action (within-subject contrast).

H5 In the HC, subjects who follow their own signal, s_2, more often might show greater mentalizing-associated activation than subjects who tend to follow the first movers' actions (between-subject contrast).

It was with these hypotheses in mind that the experiment (i.e., order of the stimulus presentations, between-stimulus intervals, number of trials) was designed.[6]

3.3 Analysis of subjects' behavior

3.3.1 Introductory remarks

In our analysis of second players' behavior, we first investigate which influence our experimental manipulation (i.e., the two different conditions and the varying signal qualities) had on subjects' choices. We then take a detailed look at

[6] To design an experiment based on rather specific prior hypotheses is common practice in neuroimaging studies. The reason for this is that the statistical analysis of brain activation data is very involved, and one has to make sure beforehand that one can actually find an effect in case one's hypotheses are true.

their response times and how response times depend on subjects' choices, because this provides information on whether it is beliefs alone or an interaction of beliefs and preferences that shapes subjects' behavior.

All results reported in this section are based on a 5% significance level if not stated otherwise.

The behavioral analysis is later complemented by an analysis of the fMRI data (Section 3.4).

3.3.2 Adequacy of the task

On average, the 34 subjects did not respond in only 0.76 out of 210 periods (0.36% of all cases). This, together with the average response time of 2.08 seconds, indicates that the permitted maximum response time of 6 sec was sufficient and that the experiment was not overly exhausting. Furthermore, subjects seem to have been sure about the vast majority of their choices, since the opportunity to correct choices was used in only 1.28% of cases.

3.3.3 Choices

Trials in the computer condition and with identical hints in the human condition

Let us begin by examining the trials in the computer condition as well as the trials in the human condition with identical hints. For these trials, we entertain a clear prediction for subjects who maximize expected payoffs (henceforth, rational subjects). In the computer condition, maximization of expected payoffs means to obey the first signal, because it is of superior quality. In the trials of the human condition with identical hints, choices should be in line with these hints.

Nine of our 34 subjects always opted for the rational decision in these trials. Of the remaining subjects, 16 did not make more than five irrational decisions (out of 104), and the maximum relative frequency of irrational choices by a subject was 25%. Therefore, with respect to the considered trials, subjects seem to have attempted to maximize expected payoffs, but did not behave completely rationally (the overall rate of irrational choices was 5.22%).

Trials with contrary hints in the human condition

Let us now turn to the trials in the human condition where the first player's decision contradicted the private signal (i.e., "contrary hints"). If our subjects assumed that first players did not make any errors, they should have unex-

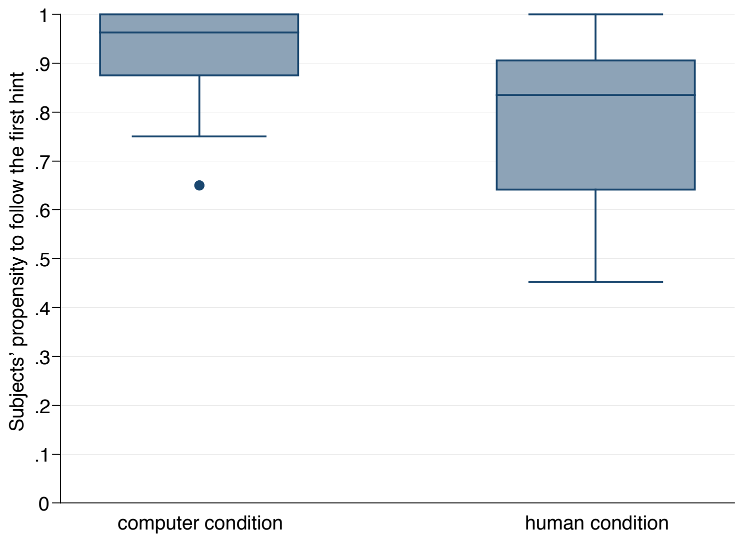

Figure 3.4: Choices in trials with contrary hints: computer vs. human condition (horizontal box lines indicate quartiles, whiskers indicate 5% and 95% quantiles).

ceptionally followed first players' choices.[7] However, only 6 out of 34 subjects displayed this kind of behavior.

In a comparison of the trials with contrary hints in the computer condition and the human condition, a paired t-test revealed that subjects' propensity to follow the first hint was significantly lower in the human condition (78.45%) than in the computer condition (92.89%, $p < 0.0001$, see also Figure 3.4). Moreover, on the individual level, 16 out of 34 subjects showed significant behavioral differences between the two conditions.[8] In each of these cases, the first hint was followed significantly less often in the human condition than the first signal in the computer condition.

Furthermore, in the trials of the human condition with contrary hints, we found that the frequency at which subjects followed the first player depended on the qualities of the signals. A probit regression with random individual effects shows that with increasing accuracy of the first hint, p_1, subjects' propensity to follow the first player increased significantly, while an increasing accuracy of the second signal, p_2, weakened the propensity to follow the first

[7] In fact, a look at the first players' behavior reveals an error rate of 8.6% in the human condition. This is even higher than that of the scanned subjects, although the first players faced an even simpler decision situation in the human condition. However, this is mainly due to a few first players who showed high rates of irrationality; 9 out of the 16 first players acted rationally without any exception.

[8] Results are based on Fisher's exact test for each individual. If not stated otherwise, all tests reported in this section use a significance level of 5%.

Pr[follow first player]	Coeff.	Std. error	$P > \|z\|$	95% conf. interval	
p_1	23.31	1.09	0.000	21.17	25.45
p_2	−22.78	1.09	0.000	−24.92	−20.63
constant	−0.59	0.42	0.158	−1.41	0.22

Table 3.2: Effect of signal qualities on subjects' propensity to follow the first player (human condition with contrary hints).

player (see Table 3.2). Using a Wald test, we cannot reject the hypothesis that the sum of both coefficients is zero.

Probit regressions on the individual level for those 28 subjects who followed their private signal s_2 at least once corroborate these findings: Except for one of these subjects, p_1 had a significantly positive and p_2 had a significantly negative effect on their propensity to follow the first player. Furthermore, Wald tests reveal that only 9 of these subjects showed a difference between (the absolute values of) both coefficients, which indicates that the difference of the signal qualities $\Delta p \equiv p_1 - p_2$ determined subjects' behavior. Figure 3.5 illustrates this relationship over all subjects.

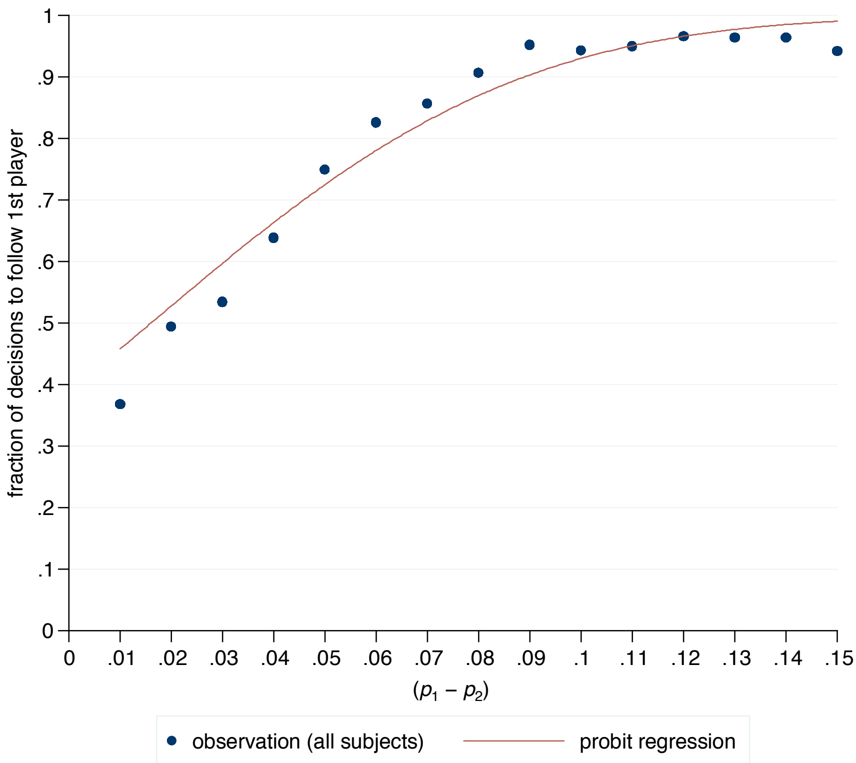

Figure 3.5: Effect of the difference in the signal qualities on the propensity to follow the first player (human condition with contrary hints).

Hint congruency	Computer condition	Human condition	Average
Identical hints	1.649	1.809	1.759
Contrary hints	2.016	2.294	2.218
Average	1.894	2.151	2.078

Table 3.3: Response times [in sec] by conditions and hint congruency.

Response time in ms	Coeff.	Std. error	$P > \|z\|$	95% conf. interval	
Human condition (HC)	0.161	0.039	0.000	0.085	0.237
Contrary signals	0.366	0.040	0.000	0.288	0.443
HC × Contrary signals	0.118	0.047	0.012	0.026	0.211
constant	1.650	0.121	0.000	1.413	1.886

Table 3.4: Effect of human condition and contrary signals on subjects' response times [in sec].

3.3.4 Response times

Influence of trial characteristics (exogenous manipulation)

According to the conventional model—i.e., agents behave rationally and assume perfect rationality also on the side of other players—there is no reason to expect any differences in the response times between the human and the computer condition or between trials with identical and contrary hints. Table 3.3 presents the average response times for each of the four trial types.

Wald tests based on the panel regression with random individual effects reported in Table 3.4 revealed significantly different response times for each of the four trial types on the aggregate level. Both the presence of contrary signals and the human transmission of the first signal resulted in significantly longer response times.

On the individual level, only seven subjects showed no significant differences at all in their response times between the four trial types.[9] Remarkably, in every single case where we observe significant results on the individual level, the effects of human condition and contrary hints were positive.[10]

These findings indicate that subjects found it harder to decide—and probably engaged in more complicated deliberation—in the human condition than in the computer condition as well as in the presence of contrary hints.

[9] Five subjects acted completely rationally in the human and the computer condition, in the sense that they always followed the first hint. Four of these five subjects are among those who did not show any significant differences in response times between the conditions.

[10] Number of subjects with significant effects of contrary signals in the computer [human] condition: 11 [24] of 34 subjects; number of subjects with significant effect of human condition, given identical [contrary] hints: 5 [14] of 34 subjects.

Response time	Coeff.	Std. error	$P > \|t\|$	95% conf. interval	
Δp	−4.539	0.429	0.000	−5.380	−3.697
$D_{\neg\text{follow}}$	−0.114	0.072	0.115	−0.255	0.028
$\Delta p \times D_{\neg\text{follow}}$	7.700	1.278	0.000	5.196	10.204
constant	2.563	0.118	0.000	2.332	2.793

Table 3.5: Effects of difference in signal qualities and decision on response times [in sec] (human condition with contrary hints).

Influence of subjects' decisions (endogenous)

Focusing on the trials in the human condition with contrary hints, we find that the response times in trials in which subjects chose to follow their private signal ($\overline{RT}_{H_2} = 2.77$ sec) were significantly longer compared to trials in which they chose to follow the preceding player ($\overline{RT}_{H_1} = 2.16$ sec).[11] Thus, it took subjects on average more than 0.6 sec (or more than one quarter of the mean response time, 2.29 sec) longer to reach a decision when the outcome of that decision was not to follow the preceding player.[12] t-tests on the individual level show that the response times differed for 14 out of the 28 subjects who followed their private signal at least once, and in each of these cases subjects needed more time when they obeyed their private signal than when they decided to follow the preceding player.

However—as in the in the analysis of choices—the signal qualities turn out to be a major determinant of the response times. Wald tests based on a panel regression with random individual effects (reported in Table 3.5) reveal that subjects' response times decreased with larger differences in the signal qualities when they chose to follow the first player, while their response times increased with larger differences in the signal qualities when they chose to follow their private signal 2 (both $p < 0.01$, see also Figure 3.6).[13]

Taken together, subjects' choices and this pattern of response times indicate that a deliberation process was going on, into which the difference in the signal qualities, Δp, entered as a suggestion which of the two options to choose. Sometimes, subjects overwrote that suggestion: in most trials, they chose to follow the preceding player when Δp was large, but sometimes they didn't. This process of overcoming the intuitive suggestion increased the response time. The same holds for small Δp: A small Δp seems to suggest that one should

[11] Result based on a panel regression with random individual effects with the response time as the dependent variable and a constant as well as a dummy for trials in which subjects followed their private signal as the independent variables ($p < 0.001$).

[12] The results are qualitatively unchanged when we restrict the analysis to the 28 subjects that followed their private information at least once.

[13] Restricting the analysis to those subjects who did not follow the first player in every trial does not change the results qualitatively.

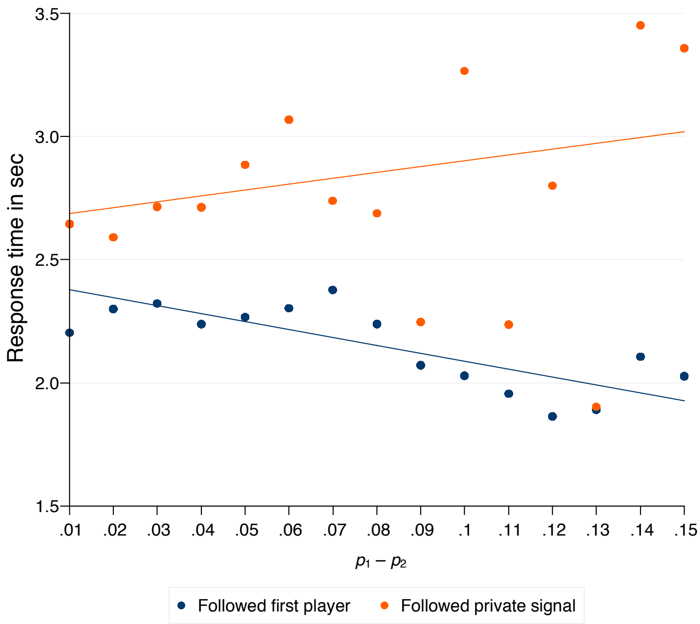

Figure 3.6: Effects of difference in signal qualities and decision on response times [in sec] (human condition with contrary hints).

rather obey one's own private signal. Overturning this suggestion increased the response time.

Our regression implies that it took subjects on average longer to decide when they obeyed their private signal than when they followed the preceding player. What might be the reason for this?

One possible explanation is that it seemed "more natural" to subjects to do what someone else had done before—a desire for social conformity (see Goeree and Yariv, 2007) that has to be overruled when not following the preceding player. Another possible explanation is that subjects' choice to follow the other player developed into a default option merely due to the fact that this was the choice made in most trials (78.45% of the trials in the human condition with contrary hints).

In order to rule out the latter explanation, we can as a first step look at those differences of the signal qualities for which subjects' frequency of choices to follow their private signal was not significantly different from 50%. Conducting this test for each difference of signal qualities, Δp, separately reveals that the relevant frequency is not significantly different from 50% for all $\Delta p \leq 0.03$ (compare Figure 3.5).[14] Focusing on these observations, we find that subjects' response times were still significantly longer when they decided to obey their

[14] Results based on probit regressions with random individual effects containing a constant only.

private signal ($\overline{RT}_{H_2} = 2.65$ sec) than when they chose to follow the first player ($\overline{RT}_{H_1} = 2.28$ sec).[15]

A more convincing test is the following: We can compare the response times between choices for subjects whose fraction to follow their private signal is statistically not distinguishable from 50% for all Δp in the trials with contrary hints in the human condition. Six subjects fulfill this criterion.[16] For these subjects, again, we find that their response times when following their private signal ($\overline{RT}_{H_2} = 2.78$ sec) is significantly higher than when following the first player ($\overline{RT}_{H_1} = 2.26$ sec).[17] Note that for both restricted analyses the difference of response times as well as their absolute level is very close to the respective magnitudes in the aggregate data.

Thus, we conclude that subjects' increase in response times when not following the first player was *not* due to the fact that the latter became an entrained default action over the course of the experiment.

3.3.5 Estimation of subjects' error rate beliefs

Our findings are in line with previous experimental results (Nöth and Weber, 2003; Kübler and Weizsäcker, 2004; Goeree et al., 2007) showing that subjects tend to devalue the information of preceding players, compared to a benchmark conventional theory.

This is further supported by subjects' answers in our post-experimental questionnaire: the vast majority (32/34) stated that they believed first players made errors. Such belief is justified, since first players indeed did not obey the single signal they received in 8.6% of cases.

What would a rational decision maker who thinks that first players committed errors do? If subjects hold beliefs in the form of a unique error rate $\varepsilon \in [0,1]$ that they ascribe to first players, we expect them to follow the first player whenever

$$\begin{aligned}
& \Pr[H_1 = \omega \mid H_1 \neq H_2, p_1, p_2, \varepsilon] > \Pr[H_2 = \omega \mid H_1 \neq H_2, p_1, p_2, \varepsilon] \\
\iff \quad & \Pr[H_1 = \omega \wedge H_1 \neq H_2 \mid p_1, p_2, \varepsilon] > \Pr[H_2 = \omega \wedge H_1 \neq H_2 \mid p_1, p_2, \varepsilon] \\
\iff \quad & (1-p_2)\{p_1(1-\varepsilon) + (1-p_1)\varepsilon\} > p_2\{p_1\varepsilon + (1-p_1)(1-\varepsilon)\} \\
\iff \quad & \Delta p \equiv p_1 - p_2 > \varepsilon(2p_1 - 1), \qquad (3.1)
\end{aligned}$$

[15] Result based on a regression of response times on a dummy variable that indicates subjects' choice to follow their private signal with standard errors corrected for observational clusters ($p = 0.025$). The result also holds if we exclude the case $\Delta p = 0.01$, for which subjects' frequency of following the first player (37.9%) was rather different from 50%.

[16] Results based on binomial tests for each individual.

[17] Result based on a regression of response times on a dummy variable that indicates subjects' choice to follow their private signal with standard errors corrected for observational clusters ($p < 0.001$).

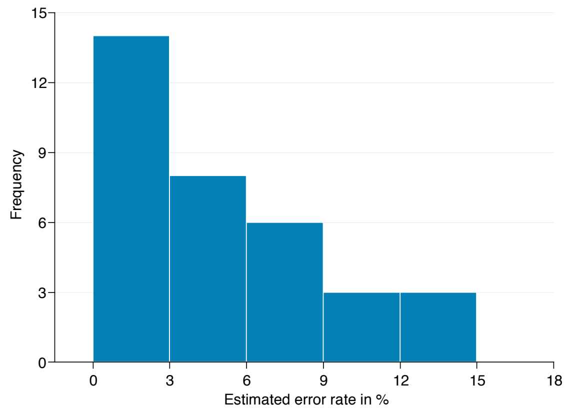

Figure 3.7: Histogram of the estimated individual error rate beliefs.

where $\Pr[H_i = \omega \mid H_1 \neq H_2, p_1, p_2, \varepsilon]$ denotes the probability that hint H_i corresponds to the true state of the world ω, given

1. the signal qualities p_1 and p_2,
2. the agent's belief about the first player's error rate ε, and
3. that the agent's second hint H_2 (her private signal s_2) contradicts her first hint H_1 (the first player's action).

Hence, if a subject ascribes a sufficiently large error rate ε to the preceding players, we would expect an increase in the subject's propensity to follow the first player for decreasing differences of the signal qualities, Δp. This is exactly in line with our findings.

In order to account for these behavioral findings in the subsequent analysis of the fMRI data, we estimated ε for each subject. In the estimation, we assumed that a subject's decision to follow the first player constituted a noisy best response (i.e., perfect rationality of our subjects is *not* assumed) to her belief about the informational content of the first player's action. Specifically, for each trial t, we assumed that an agent's probability to follow the first player, r_{H_1}, is given by

$$r_{H_1}(p_1, p_2, \varepsilon, \lambda) \equiv \Phi\{\lambda(\Pr[H_1 = \omega \mid H_1 \neq H_2, p_1, p_2, \varepsilon] - 0.5)\}. \tag{3.2}$$

λ governs the agent's response precision: For $\lambda \to \infty$, the agent always best-responds in the sense that she follows the first player whenever $\Pr[H_1 = \omega \mid H_1 \neq H_2, \varepsilon] > 0.5$ (and follows her private signal otherwise). For $\lambda = 0$, the agent chooses randomly such that each stock is chosen with probability $\frac{1}{2}$.

In order to estimate ε, we consider the sum of squared errors

$$SSE(\varepsilon, \lambda) \equiv \sum_{t=1}^{106} \{D_{H_{1,t}} - r_{H_1,t}(p_{1,t}, p_{2,t}, \varepsilon, \lambda)\}^2, \tag{3.3}$$

where $D_{H_1,t}$ is a dummy variable that indicates whether the subject followed the first player in trial t.

Minimizing (3.3) with respect to ε and λ (non-linear least-squares estimation) for each of the 28 subjects who followed their private signal at least once, we find that $\hat{\lambda}$ is significantly larger than zero for all subjects but one. Once more, this confirms that subjects' decisions depended on the (believed) informational content of the other player's decision relative to their private signal.

Regarding the estimates of ε, we find that the majority of subjects' $\hat{\varepsilon}$ is significantly larger than zero (20 of 28 subjects).[18] Figure 3.7 gives an overview of the distribution of $\hat{\varepsilon}$, showing that it ranges from virtually 0% to 13.6%.

3.4 Analysis of the fMRI data

3.4.1 Analysis 1

Regression

In the analysis of the fMRI BOLD signal, we followed the lead of the behavioral results. That is, in a first step, we analyzed whether the observed differences in subjects' behavior between the human and the computer condition were accompanied by detectable differences in brain activation. For this reason, Analysis 1 included the following six explanatory variables:[19]

1. *Human condition*;
2. *Computer condition*;
3. *Response left*;
4. *Response right*;
5. *Instruction screen*;
6. *Error trial.*

The explanatory variables took on the shape of boxcar functions[20] whose onset and duration were determined as follows: Explanatory variable 1 (*Human condition*) was on only during trials in the human condition, and off otherwise. Likewise, explanatory variable 2 (*Computer condition*) was on only during trials in the computer condition, and off otherwise. In both cases, we also conditioned the variable on subjects behaving rationally. As explained before,

[18] And that subjects make errors themselves: For the median signal qualities ($p_{1,0.5} = 0.75$ and $p_{2,0.5} = 0.69$) and median parameter estimates $\hat{\lambda}_{0.5} = 21.90$ and $\hat{\varepsilon}_{0.5} = 0.05$, the estimated frequency of following the first player is $\hat{r}_{H_1}(p_{1,0.5}, p_{2,0.5}, \hat{\varepsilon}_{.5}, \hat{\lambda}_{.5}) = 0.96$ ($\Pr\left[H_1 = \omega | H_1 \neq H_2, p_{1,0.5}, p_{2,0.5}, \hat{\varepsilon}_{.5}\right] = 0.55$).

[19] The necessary pre-statistics processing steps are explained in the appendix to this chapter (p. 82).

[20] A "boxcar function" is a step function that, in our case, represents a time dummy: It takes on the value 1 ("on") during specified time periods and 0 ("off") otherwise.

rational behavior here means that subjects obeyed the signal with the higher accuracy in the computer condition and that they did not contradict the hints if both hints were identical.[21]

The onset of the boxcar functions for the two explanatory variables was aligned to the beginning of a trial, and the boxcar function's duration in a trial was given by the time until the subject's final response (i.e., the last button press) in that trial.

Explanatory variables 3, 4, and 5 (*Response left*, *Response right*, and *Instruction screen*) were included to explain as much task-related brain activation as possible, i.e., to reduce residual variance. The onset of the *Response* boxcar functions was aligned to the moment in which subjects pressed the respective button, and their duration was set to 100 ms. The boxcar function for the *Instruction screen* explanatory variable was on whenever subjects were displayed written instructions on screen via the video goggles.

Explanatory variable 6 (*Error trial*) was on in any trial in which subjects did not behave rationally (i.e., they did not obey the signal with the higher accuracy in CC or contradicted identical hints in HC) or did not respond at all (included to explain residual variance).

All subject-specific regressors of interest were generated by convolving the respective boxcar functions with a double-gamma hemodynamic response function (HRF). Thus, the complete regression equation of Analysis 1 for each voxel i in subject j is as follows:

$$\begin{aligned}\mathrm{BOLD}_{i,j,t} = {} & \beta_{1,i,j}\,[D_{\mathrm{HC},t} * \mathrm{HRF}](t) + \beta_{2,i,j}\,[D_{\mathrm{CC},t} * \mathrm{HRF}](t) + \\ & \beta_{3,i,j}\,[D_{\text{response left},j,t} * \mathrm{HRF}](t) + \beta_{4,i,j}\,[D_{\text{response right},j,t} * \mathrm{HRF}](t) + \\ & \beta_{5,i,j}\,[D_{\text{instruction screen},t} * \mathrm{HRF}](t) + \beta_{6,i,j}\,[D_{\mathrm{error},j,t} * \mathrm{HRF}](t) + \\ & \varepsilon_{i,j,t},\end{aligned}$$

where t goes from 0 to 2100 sec (the duration of the experiment) in steps of 2 sec. BOLD is the dependent variable: the fMRI BOLD response measured in voxel i of subject j at time t. D denotes a dummy regressor, with the subscript indicating under which condition the dummy was 1 (and 0 otherwise).

The asterisk ($*$) denotes the convolution operator:

$$[f * g](t) \equiv \int_{-\infty}^{\infty} f(\tau)\,g(t-\tau)\,\mathrm{d}\tau = \int_{-\infty}^{\infty} f(t-\tau)\,g(\tau)\,\mathrm{d}\tau.$$

[21] The reason for this is that in "irrational" trials it is hard to interpret subjects' behavior and, thus, to assign meaning to the measured brain activation.

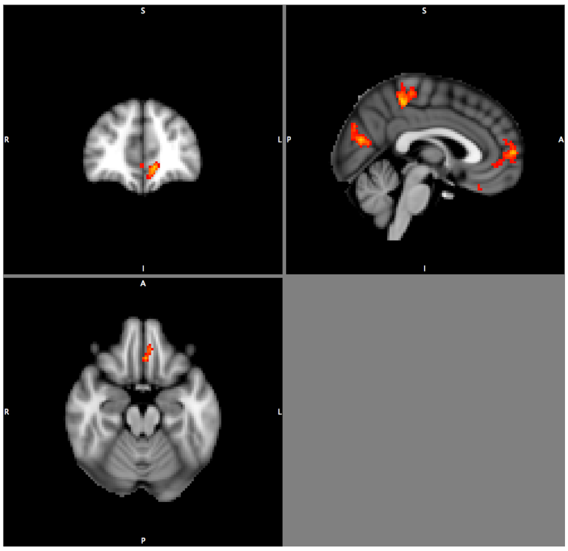

Figure 3.8: Activation foci found in Analysis 1, contrast *Computer condition* > *Human condition*, $z \geq 2.3$, cluster-corrected p-value < 0.05 (color-coded, red: $z = 2.3$, yellow: $z = 3.75$). Top right: Sagittal slice at $X = 2$ mm (MNI space). Top left: Coronal slice at $Y = 44$ mm (MNI space). Bottom left: Axial slice at $Z = -20$ mm (MNI space).

HRF stands for the assumed hemodynamic response function, for which we chose the double-gamma type in order to account for the commonly observed late undershoot of the BOLD signal (Glover, 1999).

Results

One of our prior hypotheses was that mentalizing-associated areas would be more strongly activated in the human condition than in the computer condition, see Section 3.2.5. Contrary to this hypothesis, the contrast[22] *Human condition* > *Computer condition* did not reveal any significant ($z \geq 2.3$, cluster-corrected $p \leq 0.05$) differences in activation. However, the reverse contrast showed that three areas were significantly more active in the computer condition than in the human condition. These activation foci are shown in Figure 3.8 and detailed in Table 3.6.

The ventromedial prefrontal cortex (vmPFC) is one of the areas frequently implicated in reward-based decision making. More specifically, it has recently been shown to represent the value of the decision currently made (Basten,

[22] A "contrast" is simply a comparison of two parameter estimates. When calculating a contrast, one wishes to determine whether the difference of the parameter estimates is significantly different from zero. Thus, a contrast between two conditions, calculated for voxel i, tells us whether the involved two conditions had a significantly different influence on brain activation in voxel i.

Region	Cluster size (# voxels with $z \geq 2.3$)	Cluster p-value	Maximum z-value	Peak voxel, MNI (highest z-value) X	Y	Z
Subcallosal cortex/ ventromedial pre-frontal cortex (vmPFC)	481	0.0028	3.74	−2	30	−24
Supracalcarine cortex/cuneal cortex	360	0.0180	3.51	0	−74	18
Precuneous cortex/ Posterior cingulate gyrus	337	0.0263	3.71	6	−36	50

Table 3.6: Regions found in Analysis 1, contrast Computer condition > Human condition.

Biele, Heekeren, and Fiebach, 2010). The observation that the vmPFC is more strongly activated in the computer condition than in the human condition is, thus, compatible with the explanation that subjects perceived hint 1 in the human condition to be of lower quality than signal 1 in the computer condition, because this implies that the expected reward for an average trial in the human condition is lower than the expected reward for an average trial in the computer condition. The following subsection investigates this explanation in greater detail.

What could be the reason for the fact that we do not find any mentalizing-related activation in the contrast *Human condition* > *Computer condition*? It may have been the case that our experiment did not require subjects to constantly put themselves in preceding players' shoes. It is likely that, instead, subjects actively imagined the first players' decision situation only briefly at the beginning of the experiment and that they merely performed some mental calculation in subsequent trials to determine whether it was preferable to follow or not to follow the preceding player for the announced combination of signal qualities.

Deconvolution

Using the vmPFC activation reported in Figure 3.8, we defined a region of interest (ROI) and performed a deconvolution analysis for this ROI. That is, we calculated the average BOLD signal across all voxels within this ROI, across subjects, and across all trials of a specific type (the five trial types are reported in Figure 3.9) for the seconds 0–15 following the onset of the respective trial type.

This type of analysis, thus, relies on by far weaker assumptions than the general linear model used to produce Figure 3.8. It is, in fact, complete non-

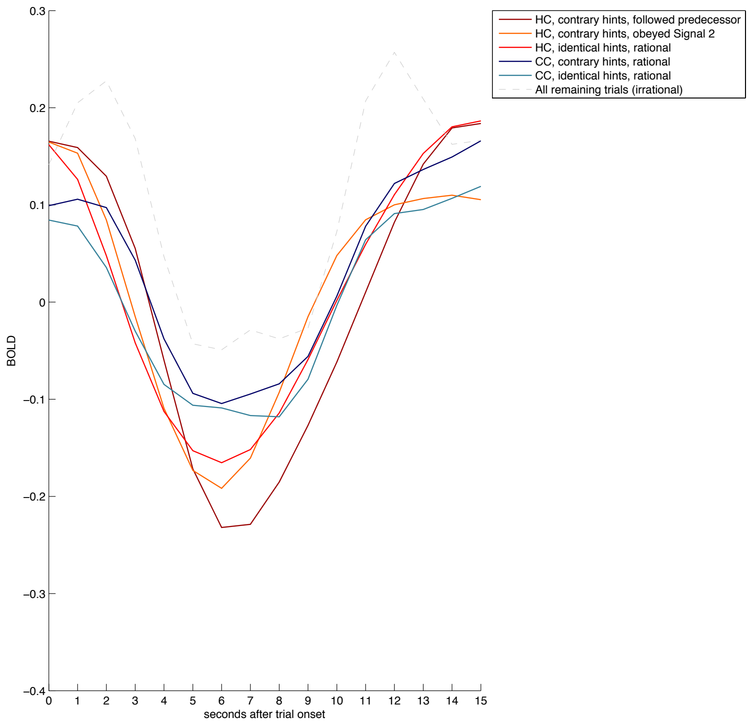

Figure 3.9: Time course of the fMRI BOLD signal [arbitrary units] in the vmPFC, within the mask provided by the activation focus depicted in Figure 3.8. Based on 28 subjects (those subjects who obeyed their private signal at least once in the human condition).

parametric. Figure 3.9 shows the estimated time course of the BOLD signal resulting from this analysis: It, of course, confirms the finding that the average BOLD response for the trials in the computer condition (CC) is higher than that for the trials in the human condition (HC).[23] This has to be the case, because the voxels within the ROI were selected exactly such that they fulfill this criterion.

A region processing expected reward should, however, fulfill additional criteria: The BOLD response for trials with identical hints should be above that for trials with contrary hints. This is the case only in the human condition, for seconds 5–7. For second 7 this difference becomes significant between the BOLD responses for *HC, identical hints* and *HC, contrary hints, followed predecessor* (paired t-test across $n = 28$ subjects).

Concerning between-subject comparisons of the activation, we should expect the following: Subjects who followed the preceding player often should have experienced less of a difference between trials with contrary hints in the human condition and in the computer condition, compared to subjects who

[23] It is common to find below-baseline activation in this area during the phase of decision making.

followed the preceding player less often. That is, we should expect the difference *CC, contrary hints – HC, contrary hints, followed predecessor*, which is on average positive, to correlate negatively with the frequency at which subjects followed the preceding player. This is indeed the case for all 15 seconds except second 6 after trial onset; the negative correlation is significant (on the 5% confidence level) for second 9 after trial onset.

Analogously, we should also observe a negative correlation for the difference *CC, contrary hints – HC, contrary hints, obeyed Signal 2*. The reason is that those subjects who follow the preceding player often, obey their own private signal (Signal 2) mostly in those cases in which its quality is close to that of Signal 1. Hence, for subjects who follow the preceding player often, the difference in expected reward between obeying Signal 2 in the human condition and obeying Signal 1 in the computer condition should be small. Again, we observe exactly this: the difference *CC, contrary hints – HC, contrary hints, obeyed Signal 2*, which is positive on average, correlates negatively with the frequency at which subjects followed the preceding player for seconds 0–11 after trial onset. However, this negative correlation did not become significant; it approached significance (i.e., $p < 0.1$) for seconds 3 and 4 after trial onset.

Taken together, while not perfect, we do find evidence that the vmPFC activation could be caused by subjects' reward expectation. An important limitation is that this analysis does not allow inference of how large the difference in the subjectively expected reward between the human and the computer condition is. Specifically, we cannot use this analysis to asses whether subjects overestimated preceding players' error rates or not.

3.4.2 Analysis 2

Regression

In a more detailed analysis, we investigated whether the additional characteristics of the experiment which we found to influence behavior were accompanied by detectable differences in brain activation. More specifically, Analysis 2 was set up

1. to, again, compare brain activation in the human condition with brain activation in the computer condition,
2. to detect whether there were differences in activation between following the preceding player and obeying one's own signal, inspired by the results from Sections 3.3.3 and 3.3.4, and
3. to find activation that correlated with subjects' individual certainty of picking the profitable stock, based on the assumed error rates that were estimated in Section 3.3.5.

Hence, Analysis 2 included the following nine explanatory variables:

1. *HC*;
2. *HC, contrary hints, obeyed own private signal*;
3. *CC*;
4. p_{decision};
5. *Response time*;
6. *Response left*;
7. *Response right*;
8. *Instruction screen*;
9. *Error trial.*

As before, these explanatory variables took on the shape of boxcar functions. Their onset was determined as before, i.e., it was aligned to the beginning of the respective trial.

However, in this regression the duration of the explanatory variables 1–4 was not set equal to the subject's response time in the respective trial but to the subject's average response time. Instead, the response time was included as a separate, parametrically modulated regressor (explanatory variable 5). As before, the explanatory variables 1–5 were on only in trials in which the respective subject behaved rationally, and off otherwise. To capture brain activation in the "irrational" trials, explanatory variable 9 was included in the regression.

p_{decision} served as a control for the influence of reward expectation on subjects' brain activation, since reward expectation might otherwise confound our previously mentioned regressors (as described in Section 3.2.2).

Again, all subject-specific regressors of interest were generated by convolving the boxcar functions with a double-gamma hemodynamic response function (HRF). Thus, the complete regression equation of Analysis 2 for each voxel i in subject j was as follows:

$$\begin{aligned}
\text{BOLD}_{i,j,t} = {} & \beta_{1,i,j}\,[D_{HC,t} * \text{HRF}](t) + \beta_{2,i,j}\,[D_{\text{HC cont, followed},j,t} * \text{HRF}](t) + \\
& \beta_{3,i,j}\,[D_{\text{CC},t} * \text{HRF}](t) + \\
& \beta_{4,i,j}\,[p_{\text{decision},j,t} * \text{HRF}](t) + \beta_{5,i,j}\,[RT_{j,t} * \text{HRF}](t) + \\
& \beta_{6,i,j}\,[D_{\text{response left},j,t} * \text{HRF}](t) + \beta_{7,i,j}\,[D_{\text{response right},j,t} * \text{HRF}](t) + \\
& \beta_{8,i,j}\,[D_{\text{instruction screen},t} * \text{HRF}](t) + \beta_{9,i,j}\,[D_{\text{error},\ j,\ t} * \text{HRF}](t) + \\
& \varepsilon_{i,j,t},
\end{aligned}$$

where t, again, goes from 0 to 2100 sec in steps of 2 sec.

The regressor *HC, contrary hints, obeyed own private signal* was centered (demeaned). Hence, the regressor *HC* captured the average activation during the human condition, while *HC, contrary hints, obeyed own private signal* captured

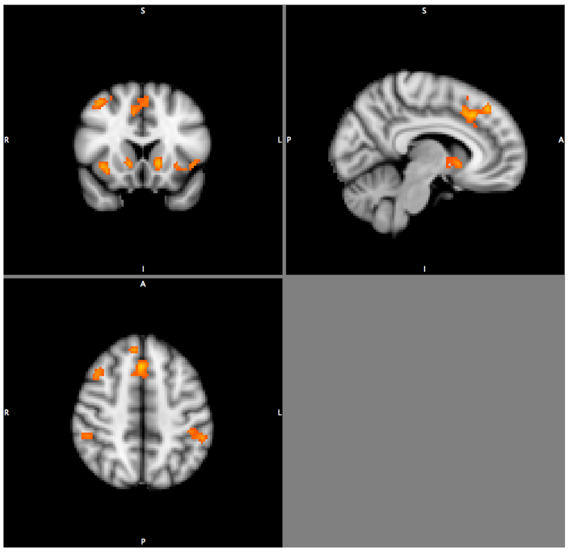

Figure 3.10: Regions found in Analysis 2, human condition, contrary hints, to show greater activation when subjects obeyed their own signal than when they followed the preceding player, z ≥ 3.1, cluster-corrected *p*-value < 0.05 (color-coded, red: $z = 2.3$, yellow: $z = 5.1$). Top right: Sagittal slice at $X = 8$ mm (MNI space). Top left: Coronal slice at $Y = 16$ mm (MNI space). Bottom left: Axial slice at $Z = 48$ mm (MNI space).

the variation around that average stemming from following/not following the first player's action.

As described in Section 3.3.5, we estimated which error rate ε subjects (implicitly) ascribed to preceding players. We used the subject-specific ε estimated this way in order to calculate for each trial the subject-specific conditional probability of making the right choice, given the two signal qualities in that trial and given ε. That is, in the case of contrary hints, we calculated p_{decision} as $\Pr[H_1 \text{ correct } (\varepsilon, H_1 \neq H_2)]$ if the respective subject followed the preceding player and as $\Pr[H_2 \text{ correct} \,|\, (\varepsilon, H_1 \neq H_2)]$ if the subject obeyed his/her private signal. In the case of identical hints, we calculated p_{decision} as $\Pr[H_1 \wedge H_2 \text{ correct} \,|\, (\varepsilon, H_1 = H_2)]$ if the subject obeyed the two hints. To let the regressor *HC* capture the average activation during the human condition, the regressor p_{decision} was also centered (de-meaned).

The *Response time* regressor was also centered, i.e., it measured the deviation from the average response time. The rationale behind including a separate regressor for the response times is the following: Response times are known to influence brain activation. We've seen in Sections 3.3.3 and 3.3.4 that not only subjects' choices correlated with reponse times, but also response times depended on the signal qualities. These signal qualities are the major determinants of p_{decision}. Thus, by including response times as a separate regressor,

Region	Cluster size (# voxels with $z \geq 2.3$)	Cluster p-value	Maximum z-value	Peak voxel, MNI (highest z-value) X	Y	Z
Dorsomedial prefrontal cortex (dmPFC)	715	6.7×10^{-11}	4.81	4	24	40
Left caudate/nucleus accumbens (NAcc)	392	4.8×10^{-7}	4.67	−12	12	−4
Right caudate/nucleus accumbens (NAcc)	164	0.00132	4.38	12	14	−2
Right anterior insula (aINS)	153	0.00208	4.66	34	20	−6
Left anterior insula (aINS)	130	0.00559	4.52	−30	20	−6
Left parietal cortex	118	0.00957	3.87	−52	−40	46
Right dorsolateral prefrontal cortex (dlPFC)	94	0.0296	3.93	38	16	52
Right parietal cortex	94	0.0296	3.54	50	−38	48

Table 3.7: Regions found in Analysis 2, human condition, contrary hints, by comparing activation when subjects did not follow the preceding player to when they followed the preceding player.

we are as conservative as possible: If the regressors *HC, contrary hints, obeyed own private signal* and p_{decision} capture brain activation despite the inclusion of the *Response time* regressor, we can be sure that this is not simply due to their correlation with response times.

Results

The activation focus in the vmPFC that was found in the contrast $CC > HC$ of Analysis 1 did not become significant on the cluster level in Analysis 2. There are still 360 voxels close to each other in the vmPFC and mPFC showing significantly different activation between the two conditions ($z \geq 2.3$), but they are not contiguous so that applying the cluster-based threshold of $p < 0.05$ renders the activation pattern insignificant.

The reason for this drop in the significance level is the inclusion of the response time regressor. It turns out that response times correlated negatively with activation in the vmPFC (significant on the 5% level, cluster-based, see Figure 3.13). Since response times were lower in the computer condition than in the human condition (see Section 3.3.4), the *Response time* regressor and the difference between the *CC* and the *HC* regressor now compete for explaining the variance in the BOLD signal.

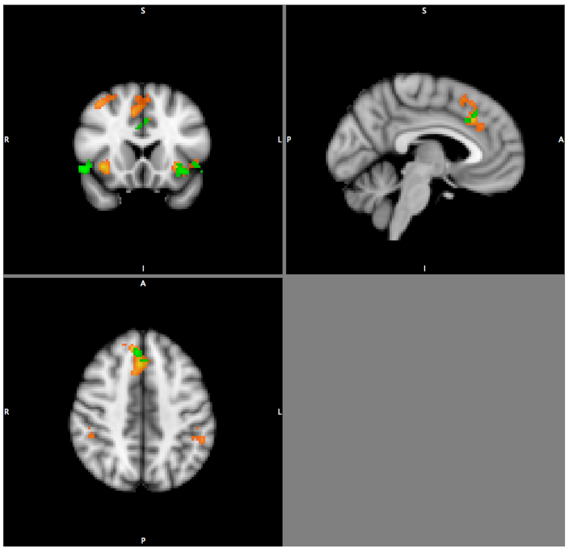

Figure 3.11: Regions in which, according to Analysis 2, the difference in activation captured by *HC, contrary hints, obeyed own private signal* covaried with the frequency at which subjects followed the preceding player, $z \geq 3.1$, cluster-corrected p-value < 0.05 (color-coded, dark green: $z = 2.3$, bright green: $z = 3.8$). Shown in the background, color-coded red–yellow, is the same activation as in Figure 3.10. Top right: Sagittal slice at $X = -4$ mm (MNI space). Top left: Coronal slice at $Y = 18$ mm (MNI space). Bottom left: Axial slice at $Z = 44$ mm (MNI space).

Interestingly, we found the most pronounced differences in brain activation not between decision situations—i.e., between human condition and computer condition or between identical hints and contrary hints within one of the conditions (our exogenous manipulation)—but depending on the decision made within one situation (an endogenous variation): Inclusion of the variable *HC, contrary hints, obeyed own private signal* reveals that an entire set of regions was more active when subjects chose not to follow the preceding player ($z \geq 3.1$, cluster-based p-value < 0.05). The regions are depicted in Figure 3.10 and listed in Table 3.7.

Importantly, no region was observed to correlate negatively with the regressor *HC, contrary hints, obeyed own private signal*, i.e., no region was significantly more active when subjects followed the preceding player than when they obeyed their own private signal. Together with the observation that it took subjects significantly longer to respond when they chose not to follow the preceding player (see Section 3.3.4), this indicates that they had to mobilize additional neural resources when *not* following the preceding player.

This is supported by the following: In the context of "Go/No-go" experiments, which investigate subjects' ability to inhibit intuitive/impulsive responses, the

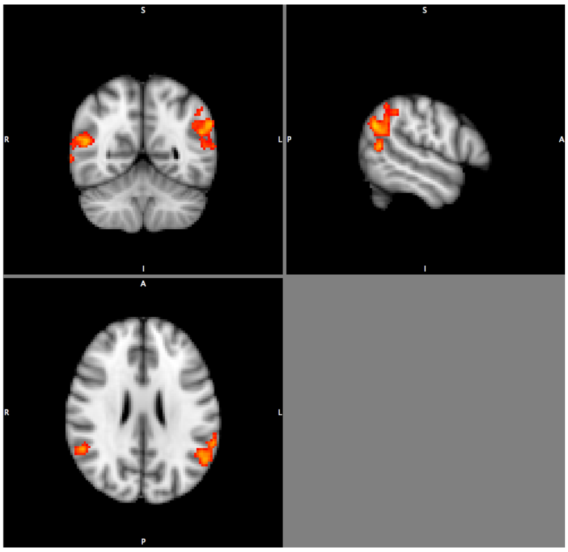

Figure 3.12: Regions in which, according to Analysis 2, activation covaried with p_{decision} (within-subject), $z \geq 2.3$, cluster-corrected p-value < 0.05. Significance level of positive correlation color-coded red–yellow (red: $z = 2.3$, yellow: $z = 3.8$). Top right: Sagittal slice at $X = -54$ mm (MNI space). Top left: Coronal slice at $Y = -56$ mm (MNI space). Bottom left: Axial slice at $Z = 26$ mm (MNI space).

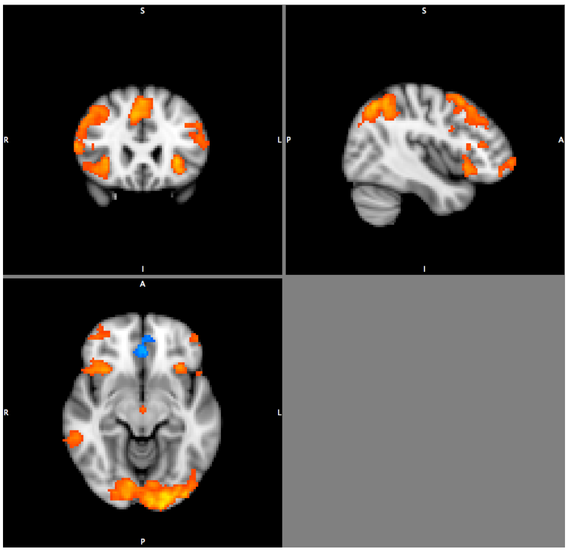

Figure 3.13: Regions in which, according to Analysis 2, activation covaried with response times (within-subject), $z \geq 3.1$, cluster-corrected p-value < 0.05. Positive correlation color-coded red–yellow (red: $z = 2.3$, yellow: $z = 4.0$). Negative correlation color-coded blue (dark blue: $z = 2.3$, bright blue: $z = 5.0$). Top right: Sagittal slice at $X = 42$ mm (MNI space). Top left: Coronal slice at $Y = 24$ mm (MNI space). Bottom left: Axial slice at $Z = -10$ mm (MNI space).

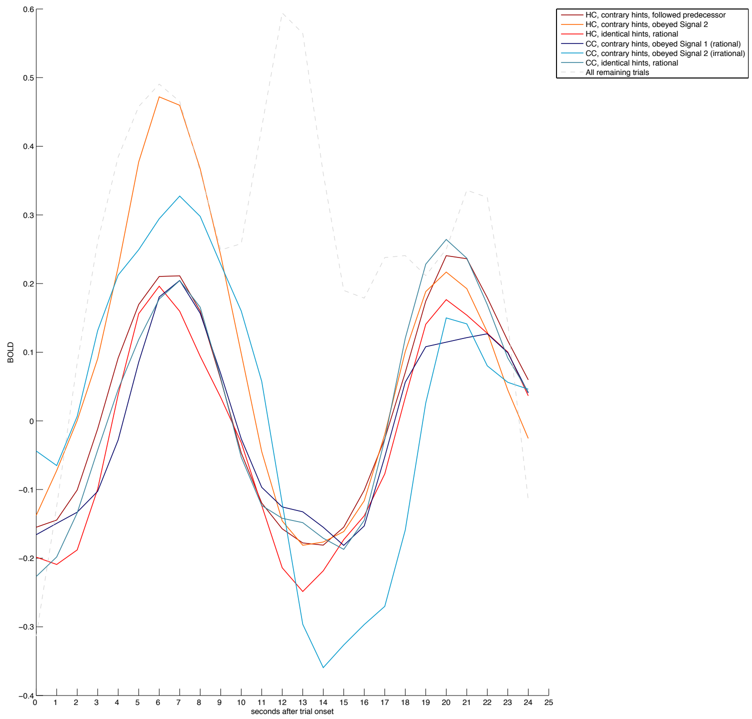

Figure 3.14: Time course of the fMRI BOLD signal [arbitrary units] in the ventral striatum, within the mask provided by the activation foci depicted in Figure 3.11.

set of regions anterior insula, putamen (part of the striatum), parietal cortex, and lateral prefrontal cortex was found by a meta-study to be activated by "successfully inhibited No-go trials" (Simmonds, Pekar, and Mostofsky, 2008). According to this interpretation, subjects had a tendency to follow the preceding player which they had to actively inhibit/overcome when obeying their own idiosyncratic signal. Such inhibition of a behavioral tendency goes along with increased response times.

This interpretation is in line with the differences in activation that we observe between-subject (depicted in Figure 3.11): Activation is the higher in dmPFC and left aINS, the more frequently subjects followed preceding players. That is, the more frequently a subject followed the preceding player, the more neurally demanding it was for her/him to overcome this tendency and to obey signal 2.

The bilateral parietal regions observed to covary significantly with p_{decision} (Figure 3.12) and also with response times (Figure 3.13) have previously been found to be activated by mental calculations (Coricelli and Nagel, 2009, p. 9166). Thus, it might be that this activation reflects the degree of difficulty of the calculations that subjects performed in a given trial.

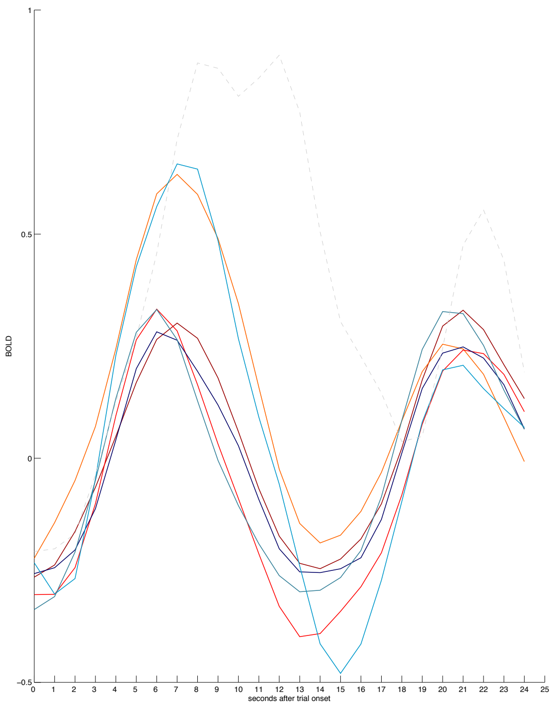

Figure 3.15: Time course of the fMRI BOLD signal [arbitrary units] in the anterior insula, within the mask provided by the activation foci depicted in Figure 3.11.

3.4.3 Deconvolution

Based on the interpretation of the activation that we find to be captured by the regressor *HC, contrary hints, obeyed own private signal* and of the response time pattern reported in Section 3.3.4, it becomes important to determine what it was that generated the neural tendency to follow the preceding player. As already discussed in Section 3.3.4, subjects' behavior might be driven by a desire for social conformity, or following the preceding player may have developed into the default option simply because subjects more frequently followed the preceding player than they did not.

Our analysis of the response times suggested that the latter can be ruled out. Do we find evidence to this effect also on the neural level?

To answer this question, we performed a deconvolution analysis on the areas which covaried significantly with subjects' choices to follow or not to follow the preceding player (Figure 3.10).

Even more than following the preceding player in the human condition, there is a clear default option in trials in the computer condition: obeying the signal with the higher quality, i.e., signal 1. Subjects chose this option in the trials with contrary hints in the computer condition in 92.89% of all cases (see

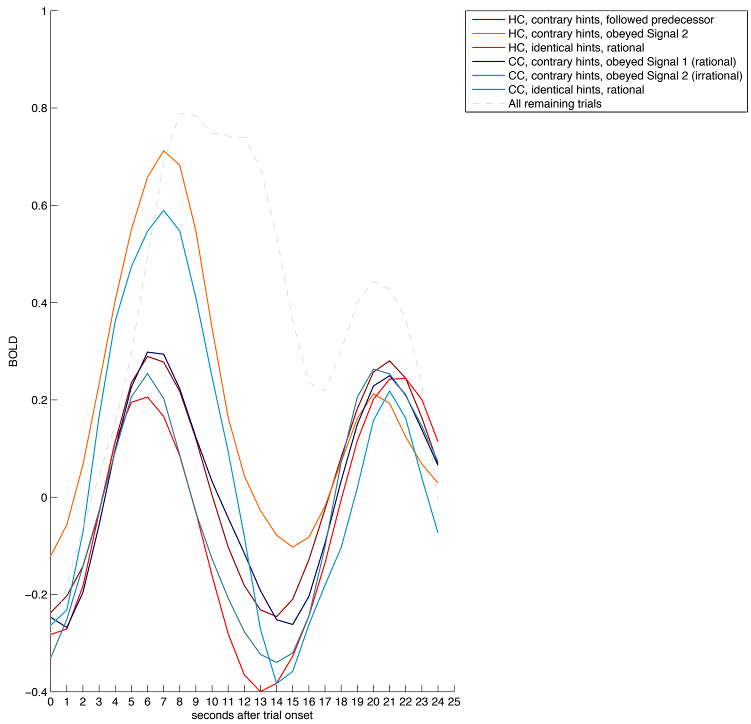

Figure 3.16: Time course of the fMRI BOLD signal [arbitrary units] in the dorsomedial prefrontal cortex, within the mask provided by the activation foci depicted in Figure 3.11.

Section 3.3.3). What happened when subjects decided not to obey the signal with the better quality in those trials—i.e., when they decided against the clearly more reasonable option?

Figure 3.14, Figure 3.15, and Figure 3.16 show that also in the computer condition, not picking the more frequently chosen option resulted in higher activation. Crucially, however, the additional activation from not obeying signal 1 in the computer condition was not higher than the additional activation from not following the preceding player in the human condition. It was, in fact, lower in the ventral striatum and the dmPFC, but not significantly so (pairwise t-test across those 14 subjects who did not obey signal 1 at least once in the computer condition). Hence, the additional activation from not following the preceding player in the human condition was likely not solely due to "following" being the default option. It seems to be the case that an additional hurdle needed to be taken when not following the preceding player—the desire for social conformity.

The time courses in the ventral striatum, anterior insula, and dmPFC display another noteworthy feature: a sharp drop (significant in insula and dmPFC) in the BOLD signal towards the end of a trial for those trials in the computer condition in which subjects behaved irrationally, compared to those trials in the human condition in which they obeyed their private signal. It was probably the case that subjects realized that they had done something unreasonable by not obeying the better signal in the computer condition and that they regretted that choice. The drop in the BOLD signal might reflect this regret.

3.5 Discussion

We had set out to shed some light on the puzzle why subjects learn too little from preceding players in social learning experiments as they have become standard in experimental economics. As a matter of fact, we ended up adding another unfitting piece to the puzzle. Both our results on subjects' response times and the results from our analyses of their brain activation suggest that subjects even have to overcome a tendency to follow the preceding player when they choose to obey their idiosyncratic signal. Thus, they seem to entertain a desire for social conformity, so that, if anything, subjects should follow others too often, not too little.

This interpretation is, however, somewhat limited by the fact that our experiment was originally designed to address a different set of hypotheses (those mentioned in Section 3.2.5). Hence, for future research we suggest performing an additional experiment that differs from our present design in the following two aspects:

1. The differences of the signal qualities, Δp, should be adjusted such that subjects individually choose each of the two options in approximately 50% of cases.
2. The order of the presentation of the two hints should be counterbalanced, such that it is not always the observed action of the preceding player that is presented first.

Let us nevertheless develop a hypothesis of what might be the reason that subjects follow others less than theory would imply—and also less than empirically optimal, as mentioned initially (Weizsäcker, 2010).

An—admittedly speculative—explanation that has not been considered in the literature so far is that subjects might consider the situation in which others make errors, at an unknown rate, as an ambiguous situation. Ambiguity aversion could then lead them to pick the alternative with the known probability of obtaining a reward—here, obeying the idiosyncratic signal with the known quality.

More specifically, suppose that agents entertain a non-degenerate prior distribution about an unkown probability (here, others' error rates) with bounded support, as modeled by Gilboa and Schmeidler (1989). Assume that agents are ambiguity-averse in the Gilboa and Schmeidler sense, i.e., they maximize minimum expected utility.

For a sufficiently large support of the non-degenerate prior, in situations with small differences in the signal qualities, Δp, the option that maximizes minimum expected utility would be to obey one's own private signal with the known accuracy. This could, in turn, lead ambiguity-averse agents to follow others less often than optimal in the face of relatively small differences of the signal qualities.

While our results do provide evidence that subjects indeed seem to assume that preceding players made errors, we cannot test this particular explanation based on our design and data. We, therefore, leave investigation of this so far speculative explanation to future research.

Acknowledgments

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Appendix: fMRI data analysis

Pre-statistics processing

Images were analyzed with the FSL software suite for fMRI data analysis (FMRIB's Software Library, http://www.fmrib.ox.ac.uk/fsl/, see Smith, Jenkinson, Woolrich, Beckmann, Behrens, Johansen-Berg, Bannister, Luca, Drobnjak, Flitney, Niazy, Saunders, Vickers, Zhang, Stefano, Brady, and Matthews, 2004; Woolrich, Jbabdi, Patenaude, Chappell, Makni, Behrens, Beckmann, Jenkinson, and Smith, 2009). Pre-statistics processing steps included motion correction, using FSL's MCFLIRT (Jenkinson, Bannister, Brady, and Smith, 2002); slice-timing correction using Fourier-space time-series phase-shifting; non-brain removal using BET (Smith, 2002); spatial smoothing using a Gaussian kernel of full width at half maximum (FWHM) 5 mm; grand-mean intensity normalization of the entire 4D dataset by a single multiplicative factor; and high-pass temporal filtering (Gaussian-weighted least-squares straight line fitting, with $\sigma = 50$ s) to remove low-frequency artifacts. fMRI data processing was carried out using FEAT (FMRI Expert Analysis Tool), version 5.98, which is part of FSL.

The motion-corrected images were co-registered to the individual's high-resolution anatomical image using a 9-parameter rigid-body transformation, and normalized to the Montreal Neurological Institute (MNI) T_1 reference brain template (resampled voxel size 2 mm × 2 mm × 2 mm), using a 12-parameter affine transformation, in order to allow for group-level anatomical localization.

Group-level analysis

We analyzed the activation data in an event-related manner by estimating several subject-specific (first-level) general linear models (GLMs)[24]; details on the different estimations are provided in the main text (Section 3.4.1).

Movement parameters derived from the realignment procedure were included in all GLMs as regressors of no interest.

[24] The model is called a *general* linear model (GLM) due to the convolution of the original explanatory variables with the HRF. Details follow in Section 3.4.1.

For all models, parameter estimates were used to calculate the appropriate contrasts. Statistical parametric maps were generated from linear contrasts of interest in each participant. After averaging across the two runs, the contrast images were entered into a t-test across all subjects (random-effects analysis).

Deconvolution

For the deconvolution analyses, the images of the activation foci used to define the ROIs were re-transformed from MNI standard space to subject space, i.e., by undoing the co-registration described above. The individual functional data were then masked by the re-transformed activation foci.

For each acquired volume, the average activation across all voxels within this mask was calculated (using the `fslmeants` command provided by FSL). Before averaging across subjects, the resulting individual average time series were standardized (made comparable between-subject) through subtraction of their respective mean and through division by their respective standard deviation over time.

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Selbstständigkeitserklärung

Ich erkläre, dass ich die vorliegende Arbeit selbstständig und nur unter Verwendung der angegebenen Literatur und Hilfsmittel angefertigt habe.

Diese Dissertation ist aus Forschungsarbeiten mit den zu Beginn des jeweiligen Kapitels genannten Personen entstanden.

Ich bezeuge durch meine Unterschrift, dass meine Angaben über die bei der Abfassung meiner Dissertation benutzten Hilfsmittel, über die mir zuteil gewordene Hilfe sowie über frühere Begutachtungen meiner Dissertation in jeder Hinsicht der Wahrheit entsprechen.

Berlin, den 9. Mai 2011 Holger Gerhardt